TRANSACTIONS

OF THE

ROYAL SOCIETY

OF

NEW SOUTH WALES,

For the Year 1871.

Sydney:
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OPPOSITE THE BANK OF NEW SOUTH WALES.

1872.

Royal Society of New South Wales.

OFFICERS FOR 1871.

PRESIDENT:

HIS EXCELLENCY THE RIGHT HON. THE EARL OF BELMORE.

VICE-PRESIDENTS:

REV. W. B. CLARKE, M.A., F.G.S.

PROFESSOR SMITH, M.D.

HONORARY TREASURER:

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HONORARY SECRETARIES:

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DR. LEIBIUS	H. C. RUSSELL, ESQ.
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CHRIS. ROLLESTON, ESQ.	CHARLES WATT, ESQ.

FUNDAMENTAL RULES.

Objects of the Society.

1. The object of the Society is to receive at its stated meetings original papers on subjects of Science, Art, Literature, and Philosophy, and especially on such subjects as tend to develop the resources of Australia, and to illustrate its Natural History and Productions.

President.

2. The Governor of New South Wales shall be *ex officio* the President of the Society.

Other Officers.

3. The other Officers of the Society shall consist of two Vice-Presidents, a Treasurer, and two or more Secretaries, who, with six other Members, shall constitute a Council for the management of the affairs of the Society.

Election of Officers.

4. The Vice-Presidents, Treasurer, Secretaries, and the six other Members of Council, shall be elected annually at a General Meeting in the month of May.

Vacancies during the Year.

5. Any vacancies occurring in the Council of Management, during the year, may be filled up by the Council.

Fees.

6. The entrance money paid by members on their admission shall be One Guinea; and the annual subscription shall be One Guinea, payable in advance.

The sum of Ten Pounds may be paid at any time as a composition for the ordinary annual payment for life.

Honorary Members.

7. The Honorary Members of the Society shall be persons who have been eminent benefactors to this or to some other of the Australian Colonies, or distinguished patrons and promoters of the objects of the Society. Every person proposed as an Honorary Member must be recommended by the Council and elected by the Society. Honorary Members shall be exempted from payment of fees and contributions, they may attend the meetings of the Society, and they shall be furnished with copies of transactions, and proceedings, published by the Society, but they shall have no right to hold office, to vote, or otherwise interfere in the business of the Society.

Confirmation of Bye-Laws.

8. Bye-Laws proposed by the Council of Management shall not be binding until ratified by a General Meeting.

Alteration of Fundamental Rules.

9. No alteration of or addition to the Fundamental Rules of the Society shall be made, unless carried at two successive General Meetings.

BYE-LAWS.

Ordinary Meetings.

1. An Ordinary Meeting of the Royal Society, to be convened by Public Advertisement, shall take place at 8 p.m., on the first Wednesday in every month, during the last eight months of the year. These Meetings will be open for the reception of contributions, and the discussion of subjects of every kind, if brought forward in conformity with the Fundamental Rules and Bye-Laws of the Society.

Council Meetings.

2. Meetings of the Council of Management shall take place on the last Wednesday in every month, and on such other days as the Council may determine.

Contributions to the Society.

3. Contributions to the Society of whatever character, must be sent to one of the Secretaries, to be laid before the Council of Management. It will be the duty of the Council to arrange, for promulgation and discussion at an Ordinary Meeting, such communications as are suitable for that purpose, as well as to dispose of the whole in the manner best adapted to promote the objects of the Society.

Ordinary Members.

4. Candidates for admission as Ordinary Members to be proposed and seconded at one of the stated meetings of the Society. The vote on their admission to take place, by ballot, at the next subsequent meeting; the assent of the majority of the Members voting at the latter meeting being requisite for the admission of the Candidate.

New Members to be notified of their Election.

5. Every Member shall receive due notification of his election, together with a Copy of the Fundamental Rules and Bye-Laws of the Society.

Introduction of New Members to the Society.

6. Every Candidate duly elected as Member should, on his first attendance at a meeting of the Society, be introduced to the Chair, by his proposer or seconder, or by some person acting on their behalf.

Annual Subscriptions, when due.

7. Annual Subscriptions shall become due on the first of May for the year then commencing. The Entrance Fee and first year's Subscription of a New Member shall become due on the day of his election.

Members whose Subscriptions are not paid to enjoy no privileges.

8. Members will not be entitled to attend the Meetings or to enjoy any of the privileges of the Society until their Entrance Fee and Subscription for the year have been paid.

Subscriptions in arrears.

9. Members who have not paid their subscriptions for the current year, shall be informed of the fact by the Treasurer. If, thirty days after such intimation, any are still indebted, their names will be formally laid before the Society at the first Ordinary Meeting. At the next Ordinary Meeting, those whose Subscriptions are still due, will be considered to have resigned.

Expulsion of Members.

10. A majority of Members present at any Ordinary Meeting, shall have power to expel an obnoxious Member from the Society, provided that a resolution to that effect has been moved and seconded at the previous Ordinary Meeting, and that due notice of the same has been sent in writing to the Member in question, within a week after the Meeting at which such resolution has been brought forward.

Admission of Visitors.

11. Every Ordinary Member shall have the privilege of admitting one friend as a Visitor to an Ordinary Meeting of the Society, on the following conditions:—

1. That the name and residence of the Visitor, together with the name of the Member introducing him, be entered in a book at the time.
2. That the Visitor does not permanently reside within ten miles of Sydney, and,
3. That he shall not have attended two meetings of the Society in the current year.

The Council shall have power to introduce Visitors, irrespective of the above restrictions.

Management of Funds.

12. The Funds of the Society shall be lodged at a Bank, named by the Council of Management. Claims against the Society, when approved by the Council, shall be paid by the Treasurer.

Audit of Accounts.

13. Two Auditors shall be appointed annually at an Ordinary Meeting to audit the Treasurer's Accounts. The Accounts as audited to be laid before the Annual Meeting in May.

LIST OF MEMBERS

OF THE

Royal Society of New South Wales.

Adams, P. F., Surveyor General.
Allen, George, the Hon., M.L.C., Toxteth Park, Glebe.
Allen, George Wigram, M.P., Elizabeth-street.
Allwood, Rev. Canon, King-street.
Allerding, F., Hunter-street.
Armstrong, Walter Dickinson, Macquarie-street.

Baker, Thomas, Maryland, near Liverpool.
Bell, Edward.
Bedford, Edward, Castlereagh-street.
Beg, Rev. Dr., Crown-street.
Beilby, E. T., Macquarie-street.
Bell, William, Pitt-street.
Belmore, His Excellency the Right Hon., the Earl of
Bensusan, S. L., George-street.
Berry, Alexander, North Shore.
Bennett, W. C., Department of Works.
Bode, Rev. G. C., Macquarie-street.
Boyd, Dr., Lyons' Terrace.
Bradridge, Thomas H., Town Hall.
Brereton, Dr., O'Connell-street.

Campbell, The Hon. Charles, M.L.C., Pine Villa, Newtown.
Cape, W. F., Pitt-street.
Cane, Alfred, Stanley-street.
Clarke, Rev. W. B., Branthwaite, North Shore.
Cox, Dr. James, Hunter-street.
Comrie, James, Northfield, Kurrajong.
Cracknell, E. C., Telegraph Office, George-street.

Cronin, J. D., Darling-street, Balmain.
Creed, Dr. Mildred, M.P., Scone.
Croudace, Thomas, Lambton.

De Lissa, Alfred, Pitt-street.
Deffell, G. H., Elizabeth-street.
Docker, the Hon. Joseph, M.L.C., Australian Club.

Elliott, F. W., Pitt-street.

Fairfax, Alfred, George-street.
Fairfax, John, *Herald* Office.
Fairfax, J. R., *Herald* Office.
Faithful, G. E., Australian Club.
Flavelle, John, George-street.
Forster, R. M., York-street.
Fortescue, Dr., Hyde Park Terrace.
Forlonge, William, Civil Service Club.
Francis, Judge.
Fraser, Collin, M.P., Australian Club, or Bannockburn, New England.

Gardiner, Martin, C.E., Gordon Terrace, Liverpool-street, East.
Garran, Dr. Andrew, Phillip-street.
Goodlet, J., 124, Erskine-street.
Gowlland, John, R.N., North Shore.
Goodchap, Charles, Civil Service Club.
Graham, Rev. John, College-street.
Gray, Samuel W., Wollumben, Tweed River, *via* Cassino.

Halloran, Henry, Colonial Secretary's Office.
Hale, Thomas, Exchange.
Hill, F. W., General Post Office.
Hill, Edward, Rose Bay. (Life.)
Hill, Roland, Joint Stock Bank.
Holden, G. K., Land Titles' Office.
Holt, the Hon. Thomas, M.L.C., the Warren, near Sydney.
Hordern, A., Darling Point.
Hovell, Captain, Goulburn.
Horton, Rev. Thomas, Hopewell-street, Paddington.
Hunt, Robert, Royal Branch Mint.

Jaques, T. J.
Jones, Dr. Sydney, College-street.
Josephson, Judge, King-street.

Kean, William, Union Club.
Krefft, Gerard, Museum, College-street.

Lang, Rev. Dr. J. D., Jamieson-street.
Leibius, Dr. Adolph, Royal Branch Mint.
Lord, the Hon. Francis, M.L.C., North Shore.
Lucas, John, M.P., Camperdown.

Macarthur, the Hon. Sir William, M.L.C.
Macafee, Arthur H. C., York-street.
Manning, John Edye, Circular Quay.
Mansfield, G. A., Pitt-street.
Mayes, Charles, Pitt-street.
McDonnell, William J., George-street.
McDonnell, William, George-street.
Metcalfe, M., Bridge-street.
Miles, Charles, 54, Upper William-street.
Mitchell, D. P., Cumberland-street.
Mitchell, James Sutherland, Tooth's Brewery, Parramatta-street.
Morehead, R. A. A., 30, O'Connell-street.
Moriarty, Edward, Department of Works.
Moore, Charles, Director of the Botanic Gardens.
Morell, G. A., Phillip-street.
Murnin, M. E., Exchange, Bridge-street.
Murray, The Hon. Sir. T. A., President of the Legislative Council.

Nathan, Charles, Macquarie-street.

Paterson, Dr., Elizabeth-street, North.
Paterson, Hugh, Macquarie-street.
Pell, Professor, Sydney University.
Phillips, Captain, Pacific Insurance Company.
Pilcher, Charles Edward, King-street.
Prince, Henry, George-street.

Ramsay, Edward, (life) Dobroyde.
Reading, E., Phillip-street.
Reed, Howard, Potts' Point.
Renwick, Dr. Arthur, Elizabeth-street.
Richardson, A. H., George-street.
Roberts, J., George-street.
Roberts, Alfred, Castlereagh-street.
Robertson, Thomas, Deniliquin.
Rolleston, Christopher, Auditor-General.
Ross, J. G., 193, Macquarie-street.
Rowe, Thomas, Pitt-street.
Russell, Henry, Sydney Observatory.

Scott, Rev. William, (life) Warden of St. Paul's College.
Scott, J. H. L., Civil Service Club.
Senior, F., George-street.

Smith, Professor, M.D., Sydney University.
Spencer, Walter W., Pitt-street South.

Tebbutt, John, Junr., Parramatta.
Thomson, the Hon. E. Deas, M.L.C., C.B.
Thomson, Professor, Sydney University.
Thompson, James, Treasury.
Thompson, H. A., Pitt-street.
Tooth, Frederick, Parramatta-street.
Tucker, William, Clifton, North Shore.
Tunks, William, M.P., North Shore.
Twynam, E., Goulburn.

Walker, William, Windsor.
Ward, R. D., North Shore.
Watt, Charles, Parramatta.
Walker, P. B., Telegraph Office, George-street.
Wallis, William, Bank Auction Rooms, George-street.
Weigall, A. B., Head Master Sydney Grammar School.
Williams, Dr., Macquarie-street.
Williams, J. P., New Pitt-street.

CONTENTS.

TRANSACTIONS

OF THE

Royal Society of New South Wales.

Opening Address to the Royal Society, delivered at its First Meeting, 12th May, 1871, by Professor Smith, M.D., Vice-President.

Your Excellency and Gentlemen,—As you did me the honour last year of electing me one of the Vice-Presidents of the Royal Society of New South Wales, it now devolves on me to give the usual opening address at the commencement of a new session.

The first meeting last year was held on May 25th. It was presided over by Mr. G. R. Smalley, Government Astronomer, who then announced that, in consequence of ill-health, he wished to retire from the Vice-Presidency. Within six weeks thereafter we had to lament his death. At the meeting held on August 3rd the following resolution was placed on record:—"That the Royal Society of New South Wales, at this, its first monthly meeting after the death of the late G. R. Smalley, Esq., desires to express its sympathy with his family, and to record in its minutes their regret at his loss, and a deep sense of the valuable services which he rendered to the society during his connection with it." Mr. Smalley was at one time in charge of the Magnetic Observatory at the Cape of Good Hope, under Sir Thomas Maclear. He afterwards taught mathematics in King's College, London; and arrived in this colony in January, 1864, as Government Astronomer, succeeding the Rev. W. Scott. He at once became an

active member of the Philosophical Society, and was mainly instrumental (in conjunction with Mr. Bedford, our present honorary treasurer,) in getting the name changed to Royal Society. Some of us thought this designation rather too ambitious, but I believe all are now satisfied that it is more appropriate than the former designation. A strictly "philosophical" society might be expected to confine its attention to matters of speculation and pure science, while in our circumstances it is expedient to devote our energies more to applied science, and to matters of obvious practical utility, not however refusing to entertain questions of speculative philosophy when competent members bring them under our notice. Following the example of the Royal Society of England, we can embrace the whole range of human knowledge and skill, avoiding only such topics as usually end in angry controversy.

While a member of the Philosophical Society, Mr. Smalley read the following papers:—"On the theory of Encke's Comet." "On certain possible relations between Geological changes and Astronomical observations." "On the present state of Astronomical, Magnetical, and Meteorological Science, and the practical bearings of these subjects." "Preliminary remarks on the Magnetical Survey of New South Wales." And in the Royal Society he read the following:—"On the Mutual Influence of Clock Pendulums." "On the value of Earth temperatures," and the opening address, session 1868. Mr. Smalley was elected one of the Vice-Presidents of the Royal Society, in conjunction with the Rev. W. B. Clarke, and continued to take a warm interest in the society till his death in July last year.

Turning now to the progress and work of the Royal Society during the year, I have to notice that we gained twenty-one new members, besides several members of the former Philosophical Society who now recognised us. In the previous year (1869) there were only fourteen new members. But while we thus had a fair addition to our numerical strength, there was no corresponding addition, I regret to say, to our income. As a consequence of this we were barely able to make ends meet, and unless we find a material improvement this year, we shall be reluctantly compelled to stop the publication of our transactions.

Our affairs are conducted on a very economical scale, and a little improvement on our annual income—which could easily be secured if our members would only make prompt payment of their subscriptions—would remove our anxieties, and enable us to continue printing the papers read before the society as hitherto.

The following is a list of the papers read during 1870:—May 25. Opening address, by the Rev. W. B. Clarke, Vice-President. June 15. "On Post Office Savings Banks, Friendly Societies, and Government Life Assurance and Annuity Offices," by C. Rolleston, Esq. July 6. "Remarks on the Report of the Water Commission, with special reference to the George's River scheme," by A. Garran, Esq., LL.D. September 19. "On the Botany Watershed," by E. Bell, Esq., C.E. November 2. "Notes on the Auriferous Slate and Granite Veins of New South Wales," by H. A. Thompson, Esq., Mining Engineer. December 7. "On the Occurrence of the Diamond near Mudgee," by Professor Thomson and Norman Taylor, Esq. Two of the regular monthly meetings were occupied by adjourned debates on the water question, and four extra meetings were held on the same debates, making altogether eight meetings devoted to the great question of Water Supply of Sydney. This was indeed the chief business of the year, and its popularity was indicated by the unusually large attendance of members and visitors on the nights of its discussion.

Dr. Garran, in his paper noted above which introduced the subject, objected to the Upper Nepean scheme recommended by the Water Commission on the ground of cost and liability to damage from the great length and variety of the works, also because it made no provision for intercepting storm waters, and because, although an embankment eighty feet high was required for the reservoir, only twenty-five feet of water would be available. He advocated the George's River scheme on the ground of nearness to Sydney, and consequent simplicity and economy of works; also, because a more capacious reservoir would be obtained, which would intercept storm waters when necessary. He maintained that an efficient dam could be constructed of loose materials, and at moderate cost, and that the salt water of the estuary would soon be replaced by fresh. Other members

during the debate supported these views. Some maintained that the cost of the Upper Nepean scheme had been greatly under-estimated, and that the water, although originally pure from that source, would become contaminated by the Wianamatta shales which it had to traverse. On the other hand, it was maintained that an effective dam on George's River would be a difficult and costly work; that if the ordinary level of the water was raised so much as ten feet, valuable land near Liverpool would be flooded; that the length of time required for washing out the salt was hypothetical; that even after the salt water was displaced, the quality of the water would never be equal to that from the Upper Nepean; and that a gravitation scheme was much more desirable than a pumping scheme. There was much incidental discussion on various other points, such as the rate of evaporation in this country, and the quality of water desirable for a town supply. In regard to the former, the weight of evidence seemed in favour of allowing four or five feet per annum; and in regard to the latter it was agreed that while water could not be too pure and soft for manufacturing purposes, yet for use as a beverage a moderate proportion of lime might be advantageous.

Mr. Bell, City Engineer, in his paper on the Botany water-shed, maintained that the Lachlan and Botany swamps were capable of affording an ample supply of water to Sydney for many years to come; that the loss by evaporation and otherwise had been greatly over-estimated by the Commissioners, and that nearly the whole of the rainfall could be made available by the construction of certain reservoirs. He was inclined to think that when it became necessary to supplement the Botany supply from some other source, the best place to go to for this purpose would be the Grose River. This paper caused a renewal of the discussion on the Upper Nepean and George's River schemes, and on various points previously handled, such as loss by evaporation, construction of dams, purity of water, and relative merits of pumping and gravitation. On the whole, this prolonged debate on the water supply, although useful in eliciting a variety of opinions and in subjecting the various schemes to an ordeal of severe criticism, yet added little to the facts as set forth in the Report of the Sydney Water Commission. It was admitted on all sides that no better water could be brought to Sydney than what is at

present supplied, and that its quantity might be increased by the construction of suitable reservoirs; but that a time would come when its capabilities could be stretched no farther, and when it would be necessary to try some other source. Opinions differed much as to when this limit would be reached. Those who believed that the time was distant advocated the expenditure of large sums in order to develope the Botany scheme to the utmost; but most who took part in the discussion were of opinion that the Botany supply would in a few years be found insufficient, and that therefore it would be inexpedient to spend more money upon it. It was argued by some that it would be better to give up the Botany stream entirely to manufactures, and to make use of the sandy and barren area drained by it as a means of disposing profitably of the city sewage; while others were disposed to maintain the Botany supply permanently, and only to supplement it from some other source. In regard to new sources, only three of those described in the water report were brought under discussion, the Upper Nepean and Cataract Rivers, recommended by the Commissioners; George's River, and the Grose. The chief objection urged against the Commissioners' scheme was that it would be much more costly than estimated, but this was not proved. On the other hand, the construction of a dam across George's River, and the purification of the water were hotly debated, and no certain conclusions could be arrived at; and as to the Grose, no champion has yet undertaken to blazon its merits, or to demonstrate a cheaper method of bringing its pure waters to Sydney than that indicated by the Commissioners.

It is, perhaps, unfortunate for Sydney that ever since the publication of the Report of the Commission, rain has fallen so abundantly that the authorities have not felt the question of water supply to be urgent. And yet it is just the time for calm and careful deliberation, for when the pressure of a great drought comes, as it certainly will come, and probably soon, the first thought will be not to find out and adopt the best plan, but to seize on the readiest. In view of the difficulties that surround the question, and the magnitude of the interests involved, I think the Government would act prudently if they were to obtain the

opinion of one or more hydraulic engineers possessing a lengthened experience in other countries; and as two gentlemen of this character have arrived from India to give advice to the Government of Victoria on the best mode of getting out of their hydraulic difficulties, it would probably not be difficult, nor very expensive, to get one or both of them to go over our contested ground here, and give us the benefit of their matured judgment.

I may notice here that the gauging of the Nepean and Cataract Rivers has been carried on since the date of the Commissioners' report, and Mr. Moriarty has kindly furnished us with a table of the monthly results, by which it will be seen that the reservoir would have been superabundantly supplied throughout the period indicated. It must be remembered, however, that last year was unusually wet.

MONTHLY TOTALS.—NEPEAN AND CATARACT RIVERS.

	Quantity of water measured as having passed down rivers.	Quantity of water run into reservoir.	Quantity of water supplied to Sydney.	Quantity of water run to waste in rivers.	Would have been available for irrigation.
	Millions of galls.	Millions of galls.	Millions of galls.	Millions of galls.	Millions of galls.
1869.					
September	1,212	973	360	239	613
October	1,091	960	372	131	588
November	2,995	1,796	360	1,199	1,436
December	770	770	372	...	398
1870.					
January	1,653	1,135	372	518	763
February	1,248	925	336	323	589
March	52,083	2,393	372	49,690	2,021
April	52,597	2,400	360	50,197	2,040
May	37,519	2,480	372	35,039	2,108
June	12,701	2,400	360	10,301	2,040
July	9,792	2,447	372	7,345	2,075
August	5,085	2,425	372	2,660	2,053
September	2,142	1,889	360	253	1,529
October	13,121	2,104	372	11,017	1,732
†	*	*	*	*	*
1871.					
April	22,156	1,604	360	20,552	1,244

† Want of funds prevented continuing observations from October, 1870, to April, 1871.

When the report on the water supply was drawn up the Commissioners had no means of determining the rainfall towards the sources of the Nepean, but they assumed that it would not be less than in Sydney, giving at the same time reasons for believing that it might be greater. After an abortive attempt to establish a rain gauge near the head of the Cordeaux, in the beginning of last year, I succeeded in starting one in that locality in July. I now give the results for ten months, along with the returns from the gauge at the University, and it will thus be seen that a great deal more rain falls on the head of the Cordeaux than at Sydney.

RAIN IN INCHES.

1870.	Cordeaux.		University.
July	4·10	...	2·90
August	2·93	...	2·57
September	2·34	...	1·41
October	14·62	...	5·14
November	13·92	...	6·62
December	11·78	...	9·00
1871.			
January	11·37	...	4·93
February	12·72	...	5·28
March	8·18	...	8·24
April	13·74	...	12·50

The consideration of water supply and rainfall leads, by an easy transition, to the general méteorology of the colony, and to the valuable paper thereupon contributed by Mr. Russell to the recently published volume on the Industrial Progress of New South Wales. Mr. Russell there shows (and I believe he is the first to do so) that there are distinct indications of a period of nineteen years in the rainfall of Sydney. I have carefully examined his diagram along with the scanty notices found in books and newspapers anterior to the regular record of rainfall, most of these being quoted by Mr. Jevons in his paper on the climate of Australia, and some being noted by myself while searching for data for my paper on the "History of the Water Supply of Sydney." Using all the facts available, I have constructed a number of tables, arranging groups of similar years under the headings "wet years," "medium years" (being those where the rainfall differs little from the mean); "medium dry years" (being those where the rainfall is under the mean, but not much); and "dry years." A mark of interrogation implies

doubt as to the character of the year. Brackets in addition to mark of interrogation imply that the character of the year is not known. When the letter *d* (dry), *w* (wet), or *m* (medium) occur after a year within brackets, it is meant that the year is exceptional or contradictory to the series. It will be seen, on examining the tables, that there are many of these exceptional years; and yet so many fall regularly within the 19-year periods that we cannot deny a certain amount of probability in Mr. Russell's theory. I cannot say, however, that I am satisfied with it; and by prolonged observation it must stand or fall.

WET YEARS.

1860	[1863 *m*]	1864	1867
1841	1844	1845	1848
1822?	1825	1826	1829?
[1803?]	1806	[1807?]	1810?
			[1791 *d*]

MEDIUM YEARS.

1857	1861	1869	[1874?]
[1838 *d*]	1842	1850	1855
1819 *w*?	[1823 *d*]	1831?	1836
1800 *w*?	[1804?]	1812?	[1817 *w*]
			[1798?]

MEDIUM DRY YEARS.

1859	[1871-2?]
1840	1852-3
[1821?]	[1833-4?]
[1802?]	1814-15

DRY YEARS.

1858	1862	1865-6	1868	[1870 *w*]	[1873?]	[1875?]
1839	[1843 *w*]	1846-7	1849	1861	1854	1856
1820	1824	1827-8	[1830?]	1832	1835	1837
[1801?]	[1805 *w*]	[1808-9?]	1811	1813	[1816?]	1818
		1789	1792?	[1794?]	1797	

In Mr. Russell's diagram of rainfall there seems to be an error in marking the year 1848, for this year in the general table stands at 59·17, making it almost identical with its 19th year (1867) which was 59·63. And there is a slight error in the annual mean. It should be 49·259, instead of 48·849. If we take the thirty complete years from 1841 to 1870 inclusive the mean comes out 49·514.

Closely connected with the subject of rain is that of winds. These afford a fine field of investigation for anyone that has a taste for meteorological inquiries, and the requisite leisure. Very little has yet been done. The daily returns from the stations

scattered over this and the neighbouring colonies should be collated and compared with the atmospheric temperature and pressure at each place. Similar returns should be procured from New Zealand and New Caledonia, and from the log-books of ships navigating the intermediate seas. Daily wind charts ought to be constructed, showing the direction and force of the wind at as many points as possible, and including readings of barometer and thermometer wherever practicable. If such laborious work were continued for some years we should then be in a favourable position for framing a theory of our winds, and for tracing the origin and progress of storms. The summer and winter winds of Sydney are on the whole so regular as to partake of the character of monsoons, and they ought to be as susceptible of satisfactory explanation as are the monsoons of the India and China seas; but so far as I know such an explanation has not yet been given. As Sydney lies outside the southern margin of the S.E. trades the normal wind ought to be north-westerly, and the winter winds of Sydney may be essentially of this character, although some modifying influence, which I am unable to indicate, causes them frequently to come from W. and S.W. When we go farther south, to Hobart Town for instance, the north-west wind blows for three-fourths of the year. The north-east wind of summer upon our coast is more puzzling. May it be a compound of return trade wind and sea breeze? The return trades ought to be north-westerly, and a true sea breeze ought to blow from about E.S.E. The former wind may give the northerly element to our summer wind, and the latter the easterly element; in other words, the return trade instead of reaching the earth's surface as a north-west wind gets turned round by reason of the heating effect of the sun upon the land, and strikes our coast from the north-east. What seems to add probability to this is the fact that during the night in summer, or rather towards morning, a land wind commonly blows from west; this dies away by 9 o'clock, and is succeeded by the north-east wind.

Lieutenant Gowlland has given a very good account of the winds on our coast, and the following are extracts from his paper:—"The prevailing winds on the coast of New South Wales may be said, as a general rule, to blow from the N.E.

during the summer half of the year (from October to March), from W. to S.W. during the autumnal and winter months (April to October). . . . . The sea breeze from N.E. during summer sets in gently in the early forenoon, preceded by a calm, with a hot, sultry atmosphere. It gradually freshens towards noon, and about the middle of the afternoon is at its height, blowing a stiff double-reefed topsail breeze, and always accompanied by a moisture in the atmosphere which is very disagreeable. . . . . During the prevalence of N.E. winds ozone is manifested in great intensity. . . . . . They nearly always lull about sunset, and if not, may be expected to blow till midnight, and then drop suddenly calm. If the barometer has been observed to fall during the previous twelve hours, they are almost certain to be followed by a "southerly burster." . . . When the glass stands high and steady at from 30·20 to 30·40, about the fall of the north-easter, little fear may be entertained of a "burster." The wind in this case falls light, veers round to N.W. and W., and blows gently off the land during the remainder of the night."

The south-easterly wind that occasionally blows for a day or two with fine weather in summer is probably the trade-wind coming farther south than usual; but the violent easterly gales that sometimes interrupt the normal winds for a few days, belong probably to cyclones. Evidences of this may often be seen in the telegraphic reports of winds and weather, published daily in the newspapers.

The well-known "hot wind" is one of the most curious of our metereological phenomena. Lieutenant Gowlland says of it, "The wind usually commences in the forenoon, and blows more or less violently until evening, sometimes lasting for two days, after which heavy black banks of clouds, charged with electricity, will be observed rapidly rising from the S.W. and southward. The wind will then veer suddenly from the hot N.W. quarter in a squall to S.W., accompanied with thunder, lightning, and heavy rain, which will continue more or less violently from three to twelve hours, finishing up from the southward, and clearing up on the following day to the S.E. and eastward. . . . . The temperature has been known to fall from 110 degs. in the shade

to 68 degs. in less than an hour." I have noticed that unless the barometer fall quickly and considerably the hot wind simply dies away and is followed by moderate southerly or easterly winds without thunder or rain.

Hot winds are scarcely known so far north as Brisbane. They are not very frequent at Sydney; and in a wet summer, like the one just passed, they are rare, and of no great severity. At Melbourne they are more frequent and more severe than at Sydney. They are felt occasionally in Tasmania; sometimes at Hobart Town without being experienced at the same time at Launceston, as, for example, on November 11, 1865, when a north-west wind at Hobart Town raised the thermometer to 95 dgrs., while there was no such wind at Launceston, and the thermometer was only 69 dgrs. Strzelecki mentions that, on one occasion, he experienced a hot wind at an altitude of 5000 feet on Ben Lomond in Tasmania, while it was not felt at an altitude of 2000 feet on the windward side of the same mountain. In this colony I have felt a hot wind on the Blue Mountains and down to Penrith, while the usual sea breeze was blowing at Sydney. Hot north-west winds occur occasionally at New Zealand, but they are said to be confined to the Province of Canterbury, where (at Christchurch) I experienced one on the 15th January, 1855. To reach that point it had to blow over the snowy crests of the neighbouring mountains, and during this wind nearly all the snow in sight from Christchurch disappeared.

In searching for an explanation of hot winds, we may consider that in the summer season there will usually be a current of heated air rising up from the surface of Australia, causing easterly sea breezes on the east coast and southerly on the south coast. But occasionally this hot upward current is overpowered and beaten back by the return trade wind, the normal direction of which is from N.W., and which for a time takes the place of the sea breezes. As the hot wind generally blows strong, it takes up impalpable dust from the earth's surface, and this dust absorbing the solar heat tends to raise still farther the temperature. The air becoming highly rarefied, causes the barometer to fall, until after a time the equilibrium is restored by a rush of cold wind from the south. More extended and accurate observations

are yet required, however, before any explanation could be accepted as satisfactory. On this subject Strzelecki remarks that "no datum is as yet offered by which we could legitimately indulge in a theory concerning the origin of this remarkable meteorological phenomenon." This excellent observer attributes to the hot wind deleterious effects on the human constitution, partaking, he says, "of the character of those produced in Egypt by the sirocco or simoom; a feverish heat and determination of blood to the head, and in those subject to disorders of the lungs a restrained action in breathing, at times bordering on suffocation; . . . relaxation of the muscles and vessels, inflammatory attacks, affections of the glottis, and opthalmia." And in the Hydrographic Notices recently issued by the Admiralty, the hot winds are said to "produce a disagreeable, dry, oppressive, enervating atmosphere, which is frequently injurious to health." I scarcely think that general experience in Sydney would bear out these opinions. Many people feel the north-east sea breeze much more relaxing and depressing than the hot winds, which indeed to some are more exhilarating than relaxing.

On the subject of the winds of this country, I have been informed that the Rev. W. B. Clarke published some letters in the *Herald* about thirty years ago. I regret that this information came to me so late that I have had no time to look up these letters, nor have I had time to consult Mr. Clarke himself, and the state of his health prevents his being present with us to-night. It is also to me a matter of regret that in the valuable paper on the Climate of Australia, published by Mr. Jevons in Waugh's Australian Almanac twelve or thirteen years ago, the chapter that he had written on the winds of Australia was omitted for want of space.

There can be little doubt that all winds are caused by differences of atmospheric pressure, these differences being due chiefly to unequal heating of the air, and to unequal distribution and precipitation of water vapour; and it may be laid down as a general rule that the wind blows from a region of higher pressure to a region of lower. But Professor Buys Ballot of Utrecht has shown that the course is not direct from the one point to the other, but that (in the northern hemisphere) the wind blows to-

wards a point on the right hand of the region of lowest pressure; and it would appear that the wind in approaching this region tends to turn round it, or to become vorticose. One of my correspondents in Scotland has recently sent out to me a rule laid down by Dr. Ballot, by which to forecast the direction of the wind on any day, from barometer readings taken on the same morning at a number of near stations. The rule is to stand with the place where the barometer is highest on your right hand, and lowest on your left, and you will have your back to the wind that may be expected during the day. This rule may perhaps require to be reversed in the southern hemisphere. The force of the wind is said to depend mainly on the amount of difference in the barometrical readings at near stations. Thus a westerly gale never blows with any severity over the British Islands unless, at least a few hours beforehand, the pressure in the north of Scotland is half an inch less than in the South of England.

It must be apparent from these observations that no satisfactory theory of our winds is likely to be attained without a much more systematic and extensive collection and comparison of facts respecting pressure, temperature, and winds than has yet been attempted, and I commend this inquiry to some active member of our Society who may be gifted with sufficient leisure and zeal.

Before concluding, I wish to call your attention to a letter that has been addressed to the President of our Society (His Excellency the Governor) by the President of the Royal Society of Victoria, on the subject of the approaching total eclipse of the sun. It is as follows:—

Melbourne, 22nd April, 1871.

Sir,—A proposition has been brought before this Society for a joint expedition from the several Australian colonies to Cape York Peninsula, to observe the total eclipse of the sun which will take place on the 12th of December next. The scientific interest of this phenomenon is very great, and the points to be determined are important. As far as is known, there will be no further total eclipse of the sun readily accessible from Australia during the remainder of the present century. The proposal is that a steamer should be chartered to start from Melbourne about the 20th of November, and, touching at intermediate points, to convey such persons as may desire to witness the eclipse. It is expected that the total cost will not exceed £25 per head of those who form the party. It is proposed that the expedition should be under the charge of the Government Astronomer, so far as the control of the steamer is necessary to prevent undue delay. A Committee of this Society has been

appointed to make preliminary inquiries; and I have to request that you will have the goodness to make the proposal known, and to inform me how many gentlemen from Sydney are likely to join it.

I have the honor to be, Sir,
Your most obedient Servant,
ROBT. L. J. ELLERY,
President of the Royal Society of Victoria.

The President of the Royal Society, Sydney.

Mr. Russell informs me that this eclipse will be total on the east side of Cape York, latitude 13·3 S., at 2 o'clock in the afternoon, and will last three minutes twenty seconds. The time required for the voyage, and the expense, will, I fear, deter many who would desire to witness this rare phenomenon; and in a scientific point of view, it seems to me that late eclipses have been so thoroughly investigated by telescopes, spectroscopes, and photography, that what remains to be done would require such delicate instruments, and such experienced manipulators, as to be beyond the reach of colonial appliances. Still it would be very desirable that a party should be on the spot, equipped as efficiently as possible; and it might not be too much to ask the various Australian Governments to bear a portion of the expense. The Home Government might have been applied to for the use of one of Her Majesty's ships to convey the party to and from Cape York without charge; but if this has not been already done, it is now probably too late. If any gentleman would desire to join the expedition, as suggested by Mr. Ellery, he will please communicate with the Secretary.

It now only remains for me to express my regret that, owing to my approaching departure on a visit to America and Europe, it will not be in my power to attend any more meetings of the Society this year. I trust you will have a pleasant and profitable session, and that members will not only countenance the meetings, but bestir themselves to provide suitable business.

Art. I.—*On the Nebula around Eta Argus, by H. C. Russell, Esq., B.A.*

(Read May 12, 1871.)

In the months of January and February of the present year I surveyed carefully, with the fine refractor of the Sydney Observatory, the stars and nebula about the remarkable variable Eta Argus. The observations I have already printed and sent to Sir John Herschel, who has shown the greatest interest in the changes which have from time to time been reported in this object, and whose beautiful monograph of 1843 enables us now to trace some of the most wonderful changes that have ever been witnessed by astronomers.

I then omitted some results of the survey, as I did not wish to give anything as evidence which might be biassed by my personal convictions. Upon these I have based the few remarks I have to make this evening. And, as the subject may be new to some of the members of this Society, a few historical notes may not be out of place.

In 1677, when observed by Halley, at St. Helena, Eta Argus was of the fourth magnitude, thence to 1751 it does not appear to have been observed, but in that year Lacaille called it second magnitude. Another long interval and Mr. Burchell—1811 to 1815—noticed that it was fourth magnitude. 1822 Fallows calls it second magnitude, and in 1827 Burchell noticed that it had increased to the first magnitude, and writing to Mr. J. Duncan, in 1827, says "I am curious to know whether any one has observed that Eta Argus which is marked as fourth magnitude, and, was always so when I was in Africa, is now of the first magnitude, or as large as Alpha Crucis." No one, however, but himself seems to have noticed it, and he did not publish the fact. When Sir John Herschel went to the Cape in 1834 he began his observations on Eta Argus, which was then about the second magnitude, and continued so up to November, 1837; on the 16th of December, when again examining the star, he was very much surprised to find it had increased to the first magnitude, and was one of the brightest stars in the heavens. This, naturally, excited his curiosity, and led him to watch it closely to the following April (1838), when his departure from the Cape prevented further observations of it. Up to the 2nd of January, 1838, it continued

to increase, and was then equal to Alpha Centauri; after this it faded gradually, and on the 14th of April was about equal to Aldebaran.

In 1843, Sir Thomas Maclear, at the Cape, observed it much brighter than Alpha Centauri, and rather brighter than Canopus, and on the 14th of March thought it almost equal to Sirius. As Sir John Herschel estimated Canopus as double, and Sirius as quadruple of Alpha Centauri, Eta was at that time probably triple Alpha Centauri; it then faded again, but in 1845 was brighter than Canopus, and had been so for some time. These observations proved the extraordinary fluctuations of the light of this star, and made it one of the most interesting objects in the heavens and the observations of various astronomers since have in no way tended to decrease the interest then excited. From their published results I have, for convenient reference, drawn up the attached list. It contains the observations from which Professor Loomis, in April, 1869, deduced the generally accepted period of 67 years, and some additional ones.

Professor Wolf, in 1863, thought that a period of forty-six years would satisfy the observations; but Professor Loomis found that subsequent observations, especially those of Mr. Tebbutt, could not be satisfied except by assuming a longer period, and gives the result of his investigation in vol. xxviii R. A. S. notices. His diagram exhibits minor fluctuations of light which may, perhaps, in some cases be accounted for by errors in the observations, but not in all; and there can be no doubt that Eta is subject to strange minor fluctuations of light in addition to its periodical variation.

Sir John Herschel, (in 1843) (at page 36 of the Cape Observations) says, "A strange field of speculation is opened up by this phenomenon—the temporary stars heretofore recorded have all become totally extinct. Variable stars, so far as they have been attended to, have exhibited periodical alterations, in some degree at least regular, of splendour and comparative obscurity. But here we have a star fitfully variable to an astonishing extent, and whose fluctuations are spread over centuries. Its future career will be a subject of high physical interest." Since 1845 Eta has gradually faded, and is now (1871) only a 7th magnitude star; less than it has ever been observed before, and, perhaps, going like all other temporary stars into darkness: certainly with its fading light throwing many dark shadows in the way of any speculations on the constitution of temporary stars.

Of the nebula about it little notice seems to have been taken for a number of years; the difficulty of drawing it would deter most observers, and the differences observed naturally attributed

to the difference in the instruments. The Cape drawing being always received as its appearance in a large telescope; for this drawing the observations extended over the years 1834, 5, 6, 7, and part of 8, and nothing was then seen to lead to the supposition that any change was going on in the nebula or stars.

It is not surprising, then, that Mr. Powell, who appears to have been the first to notice change in the nebula, attributed it to his comparatively small telescope, and did not then publish his observations. Amongst his photometric observations of Eta are found these notes about the nebula. March 23rd, 1860, "nebula about Eta Argus magnificent; April 15th, 1860, nebula much fainter than formerly; March 23rd, 1860, again, 'Eta is in a rough sketch placed outside the bright portion of the nebula.' And the lemniscate is described as a *channel.* Several entries follow, noting openness of the lemniscate on the south, and greatly diminished brightness of the nebula. April 4th, 1862, Eta Argus beautifully round and clear out of the lemniscate altogether; two patches of nebula with passage between them to the left, or preceding."

These were not published till May, 1864 (R. A. S. notices), after Mr. Abbott had published his observations; to Mr. Abbott is therefore due the credit of first publishing notice of a change in the appearance of the nebula. He had been observing the star for a number of years; but the first observation on the nebula that I can find is dated May 22rd, 1863, and runs thus: "A drawing made of the object Eta Argus quite distinct within the dark space." This was given to the Royal Society of Tasmania on the 9th June, 1863, in a paper on the "Variable star Eta Argus."

A further remark occurs in that paper to this effect: "Comparing the present description with the Cape drawing, it will, I think, appear conclusive that the apparition of the surrounding nebula is also variable." "The open space given in the Cape monograph and also in the last addition of the outlines is somewhat in the form of a dumb-bell compressed in the centre and surrounded with nebula, in the most dense part of which is situated Eta Argus. The appearance of the open space now assumes the form of a crooked billet, wide in the centre and open at both ends, with Eta Argus situated 1′ ± within the open space or dark part, and surrounded with an almost innumerable quantity of brilliant stars, some of a blue and some of a ruddy colour."

In May, 1868, some additional observations and a drawing by Mr. Abbott were published in the R. A. S. Notices. Sir John Herschel was very much interested, and carefully compared the

drawing with his own in every possible way; he could not, however, identify any of the stars, and could make nothing of the drawing.

In R. A. S. Notices for 1868, he remarks:—"It is much to be wished that some southern observer, furnished with an equatoreally mounted telescope, would without further delay, set to work and map down the stars visible within this most interesting area, down, at least, to the 10th or 11th magnitude. Possibly, I may have done Mr. Abbott injustice by assuming that his diagram is intended to convey any delineation at all of the stellar contents of his fields of view; or anything beyond the forms of the nebulous masses as existing among scattered stars. But the question once raised is of the last importance, and must be settled. The question here is not one of minute variations in subordinate features which may, or may not be attributable to differences of optical power in the instruments used by different observers, as in the case of the nebula in Orion (the only one at all comparable with it in magnitude, complexity, and brightness), but of a total change of form and character—a complete subversion of all the greatest and most striking features, which reminds us more of the capricious changes of form and place of a cloud drifted by the wind, than of anything before witnessed in the Sidereal Heavens."

In August of the same year, Lieutenant Herschel, having gone out to India in charge of one of the Eclipse expeditions, took the opportunity of observing Eta Argus nebula. At his father's request fifty of the principal stars were measured, and identified with stars in the Cape Monograph without difficulty; and several drawings were made of the nebula, showing an enclosed space near Eta, and other features much more like the Cape drawing than Mr. Abbott's. He remarked, also, the increased visibility of the nebula, but said there did not appear to be any very remarkable change in the distribution of the stars or nebula so far as the part immediately around Eta is concerned. R. A. S. Notices for 1869.

In 1869 Mr. Le Seur turned the large Melbourne reflector upon this nebula, and with the speculum, as it then was, could not recognise any nebula immediately round Eta, which was consequently thought to be in the dark space. I have not seen his drawing, and cannot, therefore, say what features he noted; before he left he repolished the speculum, and the telescope now performs much better. With it Mr. M'George has been able to see that Eta is still in the nebula; and in his drawing, which includes only the nebulous mass in which the lemniscate is situated, shows the very dense part near and north of Eta, the

enclosed space, and some remarkable minor details at a point about 17 seconds preceding and 80 seconds north of Eta, which are changing rapidly.

It thus appears that up to the end of 1870 no one had complied with Sir John Herschel's request, but that all the observers had confined their drawings to the nebulous mass in which the lemniscate is situated, and neglected the branches. My first intention was to do the same, for the difficulty in representing such a complicated nebula is very considerable, but I noticed such changes in the branches that I was induced to examine carefully all that is included in the Cape drawing, and to attempt to represent the greater part of it.

I determined last year to examine this object, believing that when such a subject is under discussion, all who have the means of furnishing information should do so, and because the Sydney refractor is in defining and light-gathering power nearer to the reflector with which the Cape drawing was made, than any which has since been directed to the object.

From many trials on close double stars, I find this instrument quite equal in defining power to the 18-inch reflector, but of course not equal to it for revealing minute stars.

Unfortunately, in August last Eta Argus was too low down to admit of any satisfactory observations, and I was obliged to defer it to January this year; when I took every favourable opportunity of observing it, and completed the drawing early in March. The observations were taken after the object had attained an altitude of 50° up to 64°.

Of the 108 stars in my list I was able to identify 104 with those in the Cape list; of the remaining 4—one, No. 25, is very small, and forms part of a triangle close to Eta, in 1834-8 it was probably hidden by the light of Eta; another, No. 32, is in the dark enclosure, and very small indeed, I am inclined to think it is variable, from comparisons made with four faint ones near Eta; another, No. 68, must, I think, have appeared since 1834-8, for it is now a conspicuous star, about 10 magnitude, and I am convinced could not have escaped Sir John Herschel's wonderfully accurate survey; and the fourth, No. 92, I have since found is No. 142 H, an error of 10 s. having occurred in recording the right ascension, which should be 135 s. instead of 145 s.

Of the magnitudes of the stars, it is beyond doubt that several of those in the Cape list have changed, and are therefore variable as well as Eta. Mr. Abbott speaks of change, but gives no particulars. Lieutenant Herschel also remarked it, but has not given particulars. Mr. Tebbutt, whose accurate observations since

1854 on Eta enabled Professor Loomis to correct the period of its variation, noted change in some of the stars in 1868, and, in a recent communication to the R. A. S., notes particularly the changes I have remarked in the largest stars, neither of us being at the time aware of the other's results.

In 1834-8 No. 71 was 6 magnitude, No. 72, 7 magnitude ; now No. 72 is fully half a magnitude larger than No. 71. So that at least one must have changed ; and it is remarkable also that the nebula has almost faded from these two stars, as from Eta. No. 105 was 6 magnitude, and No. 101, 7 magnitude ; now both are 7 magnitude, and equal to No. 71. Yet all these stars are much brighter than the other 7 magnitude stars of the Cape list, viz., Nos. 1, 2, 291, 300 : magnitude of 51 R = 403, 59 R = 844, and 1215. I would not, however, lay too much stress upon this until it is known whether all the 7th magnitudes in the Cape list were equal, for, if not, it may be that only Nos. 71 and 105 have changed.

As to the colours of the stars near Eta, they are, in my estimation, pale indeed compared with K Crucis ; and if in 1865 bright enough to merit Mr. Abbott's remark, "that although Sir John Herschel has not overdrawn the beauty of K Crux, the object Eta Argus is much more superb," they must have faded wonderfully since, for I only remarked colour in two beside Eta, and both were red. Dr. Wright, who has examined both objects with an $8\frac{1}{2}$-inch Browning reflector, failed to detect anything striking in those near Eta. I have measured several of the double stars, and have not yet found any evidence of angular motion. During my first evening's observation I carefully measured the differences in R.A. and declination of 54 stars from Eta, with a fine parallel wire micrometer ; a sheet of paper four times the size of the Cape drawing was then taken, and lines drawn on it to the same angular scale, but twice the distance apart ; upon this, the 54 measured stars were carefully laid down, and the drawing of the nebula then proceeded with. The sheet was kept on a desk near the telescope, and as each outline was traced with the telescope amongst the measured stars, it was laid down on the sheet. When, in order to define the positions of particular parts, other stars were required, these were measured, and the drawing proceeded with. The bright wires on a dark field of the micrometer were also found very useful in guiding the eye, and in three places used to measure the distance of the definite parts from Eta.

The drawing was then reduced to its present size with proportional compasses, and afterwards compared with the object under different states of the atmosphere, with moon and without moon,

and corrected until it was deemed a faithful representation of it as seen now.

On comparing this with the Cape monograph startling differences are found; not so great near Eta as some observers have thought, but far greater than any which astronomers have before witnessed. In the nebula in Orion the changes are so small as to be with difficulty made out; but here the densest part in 1834-8 is now one of the faintest, while close to this another part of peculiar form has become the brightest patch in the nebula.

Examining it more in detail, it is seen that the mass in which the lemniscate (or dark enclosed space) is situated is in general outline very much the same as it was thirty years since, and in some of its marked features exactly the same; as for instance, the definite outline beginning at 70 s.—240," and running still exactly as Sir John Herschel has described it amongst some small stars; but round the border of the lemniscate great changes in the relative brightness of the different parts of the outline have taken place, and some slight changes in the outline itself; the greatest being at 17 seconds preceding—100 seconds where one point now occupies the space of two in the Cape drawing; and it is remarkable that it is just at this point that the large Melbourne reflector reveals rapid change at the present time; the nebula also seems much brighter at this point than it was in 1834-8, judging from the parts which appear unchanged.

It is, however, at the south end and following side that the most remarkable change has taken place, this end and half the side have now become so faint, that with small telescopes no nebula is seen at all; while the other half of the same side has so much increased in brightness that it is now the most marked feature of the nebula, its outline on the preceding and north sides being very like that in the Cape drawing, while the following and south side of it form a curved line which seems faintly indicated in the same drawing; this is a very curious feature and seems to indicate that this is a nebula seen upon the fainter one. It has evidently become much brighter since 1834-8, and is, I think, one of the principal causes of the increased visibility of the nebula, being now always more conspicuous than any other part, and visible when they are lost in twilight or haze. In other respects this drawing seems to represent the same object as the Cape monograph, allowing for difference in the telescopes. The star No. 11 (664 H) is just in the edge of the nebula; and about midway between it and Eta is another star about 15 magnitude, not in this drawing or in the Cape list.

Passing now to the branches we find changes even more surprising than those already noted; I think without a parallel, and pointing

to the urgent necessity for carefully recorded observations of all such objects, and if this square degree is to be taken as a sample, promising more discoveries than have ever before been made in so small a space. At the spot + 40 seconds + 600 seconds in the Cape drawing is a mass of nebula forming one of its marked features, and particularly described by Sir John Herschel as so situated with regard to certain stars, that the least change would be apparent. Of this not a vestige is now to be seen; yet it was about as dense as that near Eta. Again, about star No. 100 (1103 H) there was a decided condensation of nebula, now it is not to be seen; and of the well-marked streams from stars Nos. 71 and 72 now nothing can be seen but a faint undefined haze, much fainter than that now about Eta. The nebulous branch, also, which terminated 150 sec. before Eta in the same parallel, now extends to over 200 sec. Some changes have, I think, taken place also at + 40 sec. + 1200 seconds; but all is there so faint that I am doubtful about it. The oval shewn in the north extreme of the Cape drawing is still the same, limited by the stars as it was then. In it I saw three minute stars; Sir John Herschel records four seen with his reflector.

Taken as a whole, this object must have increased in brightness very much, for it can now be seen in full moonlight; and in 1834-8 it was at all times invisible to the naked eye. This fact, and the great similarity in outline between the Sydney and the Cape drawing, have inclined me to think that, if the same reflector could again be turned to this object, the lemniscate would be found very little altered, and the apparent difference quite as much, perhaps more, in the increase of light at two points before indicated; as in the loss of light in other parts.

Still a great change has taken place, and since Eta has only a very small proper motion, which, with one exception, appears to be common to the stars and nebula near it, and that the latter presents no signs of resolvability, even in the large Melbourne reflector; but, as far as the spectroscope has yet been applied, is gas, like the great nebula in Orion; I am inclined to think that the mass surrounding Eta, and the lemniscate, is in reality two or more nebulæ in visual superposition; and that the force, whatever it may be, which renders them luminous, is decreasing in that in which Eta is seen, and increasing in that part north of Eta whose peculiarity in form was before remarked; for it seems more reasonable than to suppose that large nebula have had the necessary angular motion to bring them visually over other parts. A motion, be it remembered, so large that even astronomy presents us with no parallel; while, on the other hand, it is known that several nebula have faded in a few years so much that they could not be seen without the aid of very large telescopes. Of

this character was one discovered by Mr. Hind in 1852; it was observed rather bright by D'Arrest in 1853, by M. Auwers in 1858 fainter, and in 1861 he could not find it with the same telescope, $4\frac{1}{3}$-inch, or with a 6-inch. With the large refractor at Pulkowa it was seen, but very faint indeed. Another one in Coma Berenices, discovered by Sir William Herschel, was missed in 1862. D'Arrest having found two nebula which he thought new, Sir John Herschel pointed out that his father found three in the same place; subsequently M. Foucalt's large reflector revealed the missing object, but it was very faint indeed.

The extent of these faded nebula near Eta, if assumed to be at the distance of the nearest known fixed star is so great; that space for thousands of solar systems such as ours would be found without the orbits of the most distant members overlapping; yet all has to our senses ceased to be in about thirty years—perhaps had done so when the Cape drawing was made, with light which may have been still streaming in from space.

What can be the constitution of such systems we naturally ask? and science answers, wait! But before such facts, we must speculate.

Are they gas which has ceased to shine and now forms a dark veil obscuring the light of the stars? Are they innumerable suns, the centres of minor systems, which have used up all their stores of meteoric and cometic fuel, and are now frozen in darkness? Are they accumulations of meteors, such as we know are revolving round our own sun and at irregular periods darkening his surface, as in 1090, when it was during daytime dark for three hours—1208, for six hours—1547, for three days—1706, dark enough at 10 a.m. to light candles; and 1777, when "Messier" saw innumerable dark bodies passing over the sun at noon? Were they once incandescent, but now cooled down, still revolving round their common centre of gravity, now more and now less between us and it, making for us as the ages roll on a variable star, hazy at its minimum, like Eta is at the present time, and bright beyond measure at its maximum?

Or; are they inexplicable difficulties in the path of astronomers which no speculation will ever solve?

List of Recorded Magnitudes of Eta Argus.

Year.	Magnitude.	Observer.	Year.	Magnitude.	Observer.
1677	4	Halley	1858	2.3	Powell
1751	2	Lacaille		(made in same year	
1811	4	Burchell		2.1	by Abbott)
to			1859	3.1	Powell
1815	4	Burchell		(made in same year	

List of Recorded Magnitudes of Eta Argus.—(Continued.)

Year.	Magnitude.	Observer.	Year.	Magnitude.	Observer.
1822	2	Fallows		2.7	by Abbott)
1826	2	Brisbane	1860	3.0	Tebbutt
1827	1	Burchell		(made in same year	
1831	2	Taylor		3.3	by Powell, and
1832	2	Taylor		3.1	by Abbott)
	(made in same year		1861	3.6	Powell
	2	by Johnson)		(made in same year	
1833	2	Taylor		4.2	by Abbott)
1834	1.4	Herschel	1862	4.9	Tebbutt
to				(made in same year	
1837	1.4	Herschel		4.9	by Abbott)
1838	0.5	Herschel	1863	5.1	Tebbutt
1842	1	Maclear		(made in same year	
1843	0.5	Maclear		6.0	by Abbott, and
1845	1	Jacob		5.0	by Ellery)
1850	1	Gillis	1864	5.2	Tebbutt
1854	1	Tebbutt	1865	5.2	Tebbutt
	(made in same year		1866	5.8	Tebbutt
	1.2	by Powell)	1867	6.0	Tebbutt
1856	1.5	Powell	1868	6.0	Tebbutt
	(made in same year		1871	7.0	Russell
	1.0	by Abbott)			

Note added, 4th October, 1871.

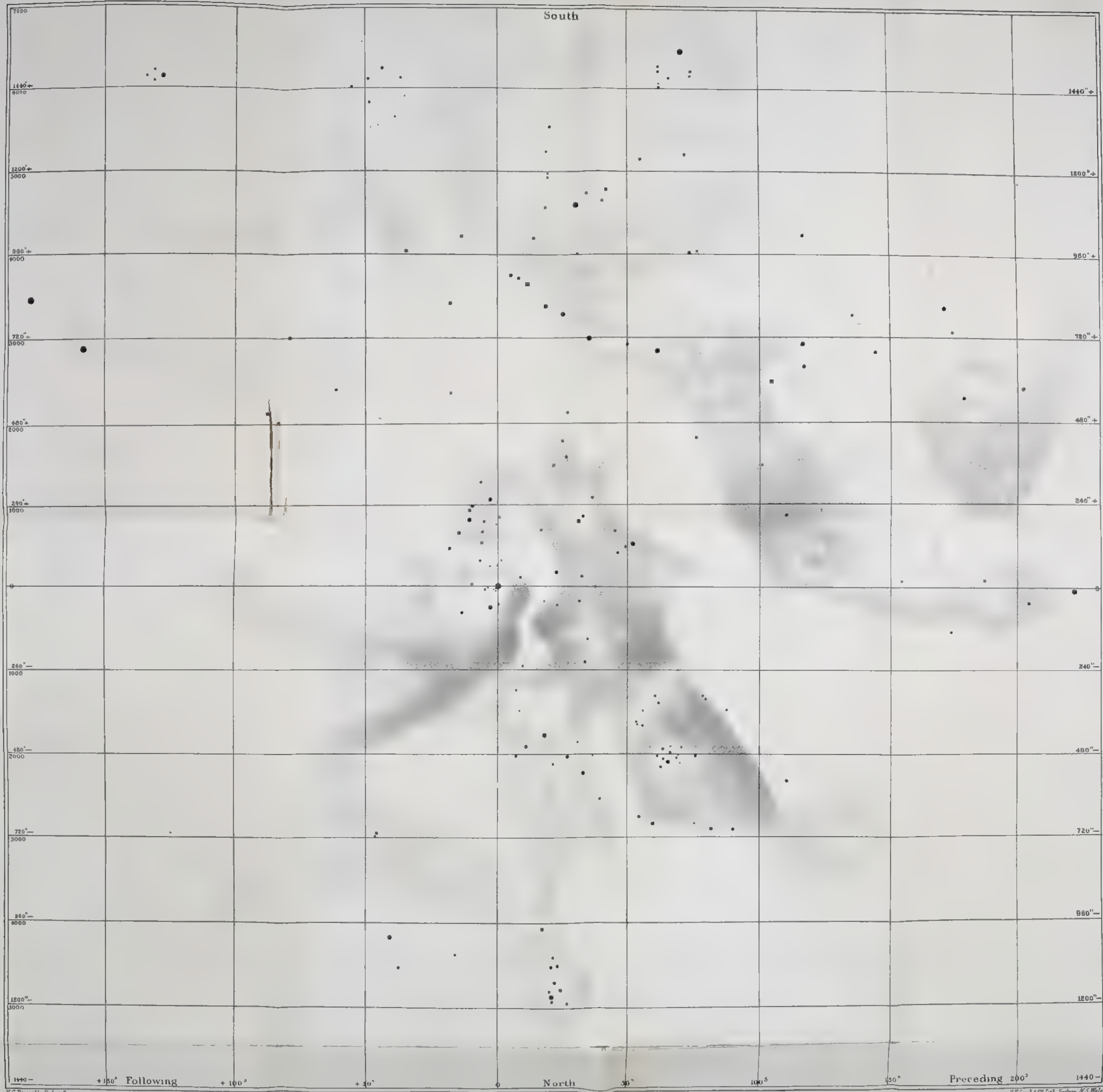
On the 5th of September, 1871, I examined this nebula with the spectroscope; owing to its unfavourable position, there was not sufficient light to enable me to make any measures, and the bright lines could only be seen when the observing room was perfectly dark: I was therefore obliged to estimate the position of the lines. When the telescope was directed to the bright portion of the nebula preceding η and divided by the line 460″ north of it, a bright green line, estimated to be the nitrogen line usually seen in nebula, and two fainter green lines on the violet side of it were seen. On the 8th of September, I again saw these lines in that part of the nebula, and in all the northern parts of the nebula which were bright enough to give a spectrum, the same bright green line was observed. I was unable to obtain any spectrum of η Argûs itself.

H. C. R.

50^{s}

Refra

71.



The Nebula about η Argûs as seen in the 10 feet Refractor of the Sydney Observatory
FEBRUARY, 1871

ART. II.—*On Magnetic Variations in New South Wales, by H. C. Russell, Esq., B.A.*

Read July 12, 1871.

FROM the foundation of the colony up to 1864, observations on the variations of the magnet at Sydney had generally shown a gradual increase of easterly variation; it then appeared that the maximum had been reached, and westerly motion had begun. Subsequent investigation has shown that such is the case, and that the westerly motion is at about the same rate as the easterly motion had been.

I purpose stating briefly our present knowledge on this subject, and putting the information already obtained in a form easily accessible, so that all who are interested in the magnetic variations at any particular time may be able at once to see if there are any records of the period, and what is the probable value to be given to them; and if none are recorded, then to find from the general curve the probable variation. It will of course be understood that the curve only gives the probable direction, and was formed by drawing lines from the several observed points after they had been marked on the diagram.

From what is to be shown presently, it will appear that the common practice of taking the average annual variation between two periods, and thence calculating the variation at another period, is by no means a safe one; and to assume that because the variation at Sydney has changed from an easterly to a westerly motion, therefore the same change has taken place all over Australia, would be contrary to experience in other parts of the world.

From the observations which have been made at London and Paris, since 1580, it appears that although the variation at the two places was decreasing, and the magnetic coincided with the true meridian at London in 1657, the same coincidence did not take place at Paris until 1669, or 12 years later. A similar difference is noticeable here, for while the easterly increase ceased in Sydney in 1858-9, it did not cease at Melbourne until 1867 (8 years.)

It has also been shown that in Europe the variation may alter as much as 18′ in one year, and at other times alter only 46′ in 35 years, or $1\frac{1}{3}'$ per annum; at London the annual change of

variation became gradually accelerated from $7\frac{1}{2}'$ per annum, in 1580, to $13\frac{1}{4}'$ in 1723, and yet during this period, viz., at 1657, the line of no variation was passed.

Again, in 1784 the variation at St. Petersburg ceased to move to the west, at Paris the same thing did not take place until 1814, and at London until 1819, showing that the progress of the change in the western variation is from east to west, or that the change begins at the eastern station, a fact which so far has been borne out here; the change at Sydney being eight years in advance of Melbourne, and since this distance $6\frac{1}{4}°$ of longitude took 8 years, it is probable that about Wentworth the variation has still an easterly increase.

It thus appears that in the Northern Hemisphere, 35 years elapsed from the time greatest west variation was reached at St. Petersburg till the same thing took place in London, which is 30° to the west, and here 50 years passed between the change at London, and (making allowance for the difference of position with regard to the North Pole) Sydney 29° west. The decrease of variation and peculiarity of the Sydney curve from 1813 to 1823 was preceded by a similar motion of the magnet at London from 1722 to 1773, so it would appear that great changes in the magnetic variation take place at London from 50 to 70 years in advance of Sydney, although the passing disturbances are simultaneous.

We are indebted to the Rev. W. B. Clarke for making the first list of variations determined at Sydney and Parramatta, and publishing it in the "Australian Almanac" for 1858.

The first determination of the variation in Australia which I find recorded, was made by Captain Cook at Botany Bay, on 29th April, 1770. He then made it 11° 3′ east. There can be no doubt from subsequent observations that this result is considerably in error—probably too large by nearly 3°. In 1788, Lieutenant Shutland, then in command of the *Alexander*, transport, made the variation 8° 6′ east—a result which is probably too small by about half a degree. In 1793, Brewster made it 8° 46 east. In 1803, Flinders made it 8° 51′ east; the longitude, latitude, and other results obtained by this observer are so good that I am inclined to place much reliance upon this result (Flinders, vol. 1, p. 239). Brewster again, in 1813, observed at Parramatta, and the result corrected to Sydney by the difference between the two places, is 8° 51′ 34″, or a change of 34 seconds in a period of ten years. Captain P. P. King, in his voyage to Australia in 1817, made the variation at Sydney 8° 42′ east. Surveyor-General Oxley, in April, 1818, after determining the position of Macquarie Lighthouse, South Head, states that all the bearings are magnetic, and the variation 9° east. This is probably only an

approximate result. Rowe, in 1822, made it 9° 6′ east, but the three determinations made by this observer during the same visit to Sydney differed nearly 2°, or from 8° 40′ to upwards of 10°.

Brewster, in 1822, during the month of October, made the variation 8° 47′ 48″. Up to this date the time of day when the observations were taken is not stated by any of the observers, there is, therefore, an uncertainty amounting to several minutes of arc in all the results.

In 1823, Mr. Rumker, at Parramatta, made the variation at noon in April, 8° 47′ 41″ east, which corrected to Sydney is 8° 51′ 41″, this result was obtained with a small magnetic transit by Dollond. The transit could be converted at will to a telescope for observing the sun on the meridian, or a microscope to observe the end of the magnet. The supports of the transit turned on an azimuth circle, and the true meridian was obtained by making the wire bisect the sun at the instant of meridian passage, noting the reading of the azimuth circle. Changing the instrument to a microscope and observing the magnet, a second reading, giving the magnetic meridian, and the difference by taking one from the other, the variation. In 1823, probably with the same instrument, Sir Thomas Brisbane made it 8° 45′ in Sydney; the date is not given.

Brewster, 1824, during January and February, made it 8° 56′ east. Dunlop, astronomer at Parramatta, at noon in April, made it, when corrected, to Sydney, 9° 3′ 4″ east for 1825, and 9° 21′ 7″ east for September, 1832. In 1841, Sir J. C. Ross made the variation on Garden Island (Sydney Harbour), 9° 57′ 19″ east (vol. 2 page 41), being a mean of results obtained from 21st to the 28th July that year; there is strong probability that this is a misprint for 9° 27′ 19″, for two reasons, first, because the first result does not agree with any other results; and, second, because a few pages further on he gives the variation as 9° 42′ east at 30° 52′ south latitude, and 154° 8′ east longitude—that is 3 degrees east of Sydney less than at Sydney—while the variation increases to the eastward of Sydney; assuming 9° 27′ 19″ to be the correct result, it agrees very well with those of Dunlop and Admiral King.

Blackwood, in February, 1844, made it 9° 25′ east. Admiral King, at Parramatta, using the same instrument that Dunlop and Rumker had used, made it, when corrected to Sydney, 9° 42′ 52″ east at noon in April, 1851, and 9° 47′ 56″ noon in April, 1851. Captain Denham, in 1857, made it 10°, in 1858 the same; and the ship *Novara*, in 1859, 9° 59′ 54″, in December.

Government Astronomer,	1864, 9° 49′ 4″	East	
	1866, 9° 42′ 54″	,,	
	1867, 9° 40′ 40″	,,	
	1870, 9° 36′ 36″	,,	
	1871, 9° 35′ 0″	,,	February.

Diagram No. 1, was formed by projecting the whole of these results, except the two first. It is at once evident that some of the observations must have been in error, especially from 1813 to 1823, and again in 1841 and 1864. Between King and Oxley, though only a year apart in time, the difference is 18′; and between Rowe and Sir Thomas Brisbane, there is a difference of 21′, though only a year apart; and there are two intermediate results, both of which are probably good. In 1841 it is evident that the assumed variation is the true one, and the same with 1864.

A mere inspection of the curve from 1813 to 1823, combined with the fact that a mean of the different results then obtained would put the curve just where we might expect, about 8° 50′, would seem to indicate that the curve should be so drawn; but, I think it is better to retain the present curve, even though it appears irregular; for there are the satisfactory results of five observers for its present position, and only two for changing it—one of these had in all probability no delicate instrument to determine the variation, and the other obtained three most unsatisfactory results.

At the present time (July, 1871) the variation at Sydney is 9° 35′ East, and the same variation would be found all along the line joining Sydney and Cooma. Lines drawn on the map of the colony parallel to this, represent lines of equal variation throughout their length; approximately places one degree of longitude from each other will differ 21′ in variation, the variation decreasing to westward.

In other words, the variation is 21′ less for every fifty-eight miles westward of Sydney.

The greatest diurnal range of the magnet occurs in February, and amounts to 13′, the magnet pointing 6′ West of its mean reading at 9 a.m., and 7′ East at 2 p.m.

As a general rule, 7 p.m. to 8 p.m. is the best time to take the variation, or get the true magnetic medium, because at that time the magnet is quieter and deviates less than at any other time in the day.

Diagrams 2 to 13.—I have represented the average direction of the magnet at each hour of the day in each month of the year. The curves are formed from the results of hourly observations, taken at Hobart Town from 1841 to 1848, and published in the "Philosophical Society's Transactions for 1851" (paper by Lieut. Colonel Edward Sabine, R.A.) From a number of hourly observations made here, I find the magnet follows a similar curve, and I have given the results, because they will be useful to indicate the best time to take the variation; and in cases where the variation can only be observed at a particular time in the day, will

afford the approximate correction necessary to reduce the observation to the true magnetic direction. As the results are the averages for each month, they represent the direction for the middle of the month; the curve for any other time may be obtained by taking a position between the preceding and following curves, nearer to one or the other in proportion to the number of days it is from them.

The black line through each of the curves marked "variation" is supposed to represent the mean direction of the magnet for any year, and the curve shows the daily excursions of the needle east and west of that line.

Remarks on the Botany of Lord Howe's Island, by Charles Moore, Esq.

[Read before the Society, 18th October, 1871.]

In September, 1869, I made a Report to the Government on the plants collected by me upon Lord Howe's Island, in the month of June previous. In that report the vegetation, as I stated at the time, was necessarily but imperfectly represented, from the fact that I had only the very limited period of 3 days allowed me for investigation, and that up to that time very little indeed was known of the botany of the place, although the island was discovered so long ago as 1788, and since that time has been visited very frequently by those on board ships of war and merchant vessels, as well as having been inhabited by both Europeans ond others since 1833. It is strange, therefore, that with so many advantages as these for making known its Flora, greater results were not produced than have at least been published. So far as I have been able to discover, up to the time of my visit not more than about a dozen plants were enumerated or described in any botanical work as natives of the island; and yet it is on record that McGillivray, the naturalist on board H.M.S. *Rattlesnake*, and Milne, botanical collector on board H.M.S. *Herald*, visited the island officially at different times, and made collections of dried specimens, and it is principally from these sources that the very few plants described in the *Flora Australiensis* were obtained. For these reasons, I was not prepared to meet with a vegetation so new and varied in character as to enable me to collect, during my short visit, about 120 species, the greater number of which were previously unknown.

Since that time I have had several additional species to the collection then made furnished to me by settlers, and during a recent cruise of H.M.S. *Rosario*, my assistant, Mr. Carron, was enabled, through the kindness of Commodore Stirling, the Captain, and officers of that ship, to pay a second visit to the island, he having accompanied me on the former occasion, and it is partly with a view of bringing under notice the new plants discovered by him, and those sent to me by one of the settlers employed for the purpose, that I have been induced to prepare the following brief remarks, which are simply intended as supplementary to the report made by me to the Government.

I may state, that on the occasion of my visit at least two-thirds of the island (which is scarcely 7 miles in length, by an average of 1 mile in breadth) was very carefully botanically examined by myself and those who assisted me. It was scarcely to be expected, therefore, that after this, any very great number of additional species would be obtained from any part which we were enabled to reach, and such has proved to be the case, as nearly all the plants new to my collection have been obtained from the upper or mountainous region. Notwithstanding these recent acquisitions, I do not yet by any means consider the collection which has been made as a complete illustration of the Flora of the island. Better opportunities must occur for further research before a knowledge of the Botany of the place can be perfected. It is only the plants of the lower and middle regions that are known, the upper region is still a *terra incognita* to the botanist; and all that is known of its vegetation is from specimens procured by the settler previously referred to—a very illiterate man, without any particular taste or other special qualification for distinguishing one kind of plant from another. The specimens, however, procured for me through the instrumentality of this person, from the more elevated parts of Mount Lidgbird and Gower, have proved so very interesting as to make it most desirable that both localities should be thoroughly examined by some person well qualified for the work This is the more necessary, as specimens of several species obtained from these places are so incomplete in their phytological characters as to render it impossible to determine even the family to which they belong; and our ignorance of these and some plants collected by myself must remain until better materials for descriptive purposes can be obtained. The additions which have been made since the publication of my former list consist of 7 Cryptogams, and about 22 Phanerogams. Among the former class (which are all ferns) are a species of *Adiantum* (*A. hispidulum*)—a genus which I stated in my report to be altogether missing on the island—and two arborescent species; one an *Alsophila*, presenting the

novel characteristic of having adventitious branches produced from all parts of the stem—a rare peculiarity in ferns, and particularly so in the genus *Alsophila*, though it has been previously observed by me in a species of *Dicksonia*, inhabiting the northern parts of this colony, though not nearly to such an extent as in this case, which is altogether of an unique character—the other a species of *Cyathea*, the stem of which attains a height of about 8 feet, with an average diameter of about 3 or 4 inches. Its stem and stipes are perfectly smooth and shining, and the fronds, which have a wide-spreading, graceful habit, are large in proportion to the stem. From the same locality as these are a fine species of *Lomaria*, very conspicuous from the great size of the lateral pinnæ, which are auricle formed, an *Aspidium*, a *Trichomanes*, and a *Hymenophyllum*. The latter class (the Phanerogamic or flowering plants) are interesting as affording in several instances new links between those of the island and the plants of this and the adjoining countries, as will be apparent from the genera which are represented by these additions; viz., *Leucopogon*, *Eugenia*, *Metrosideros*, *Pittosporum*, *Cupania*, *Panax*, *Melicope*, *Pipturus*, *Passiflora*, and *Calophyllum*,—the latter an exceedingly pretty species of a genus (as the name implies) of beautifully foliaged plants, hitherto supposed to be tropical, as this is the first instance that any of its species have been found beyond the tropics. None of these are strictly Australian, although all, with the exception of *Calophyllum*, are found more or less in this colony; most of them have representatives in New Zealand, some in Norfolk Island, and some also in New Caledonia.

Cryptogams.	Howe's Island.	N. S. Wales.	New Zealand.	Norfolk Island.
Bryaceæ: Spiridens	1	0	0	0
Lycopodiaceæ: Tmesipteris	1	0	1	1
Filices: Adiantum	1	5	6	2
„ Alsophila	3	3	1	1
„ Aspidium	1	2	4	2
„ Asplenium	3	7	9	5
„ Cyathea	1	1	4	0
„ Davallia	1	1	2	0
„ Diplazium	1	1	0	0
„ Hymenophyllum	3	4	15	0
„ Hypolepis	1	1	3	0
„ Lomaria	3	4	11	0
„ Marattia	2	0	1	1
„ Nephrolepis	1	2	0	0
„ Polypodium	5	9	3	4
„ Pteris	2	4	9	7
„ Trichomanes	2	3	5	2
	17.—32.	14.—47.	14.—74.	9.—25.

	Howe's Island.	N. S. Wales.	New Zealand.	Norfolk Island.
Endogens.				
Graminaceæ : Chloris	1	2	0	0
,, Cynodon	1	1	1	0
,, Digitaria	1	0	1	0
,, Spinifex	1	1	1	0
,, Sporobolus	1	1	1	0
,, Stipa	1	1	0	0
Cyperaceæ : Carex	1	9	22	1
,, Lamprocarya	1	1	4	0
Pandanaceæ : Pandanus	2	1	0	0
Palmaceæ : Kentia	4	0	0	0
Amaryllydaceæ : Crinum	1	2	0	0
Iridaceæ : Moræa	1	0	0	0
Orchidaceæ : Dendrobium	3	14	1	0
,, Sarcochilus	1	3	1	0
Juncaceæ : Flagellaria	1	1	0	0
,, Juncus	1	7	10	0
Smilaceæ : Smilax	1	2	0	0
	17.—23.	14.—46.	9.—42.	1.—1.
Exogens.				
Urticaceæ : Boehmeria	1	0	0	1
,, Elatostemma	1	1	1	1
Moraceæ : Ficus	1	6	0	0
,, Morus	1	1	0	1
Euphorbiaceæ : Baloghia	1	1	0	1
,, Euphorbia	1	0	2	2
,, Hemicyclia	1	1	0	0
,, Omalanthus	1	1	0	0
Santalaceæ : Exocarpus	2	2	1	1
Thymelaceæ : Pimelia	1	6	7	1
Laurinaceæ : Tetranthera	1	1	1	0
Chenopodiaceæ : Rhagodia	1	3	0	0
Polygonaceæ : Muchlenbeckia	1	2	0	0
Nyctaginaceæ : Boerhaavia	1	1	0	0
,, Pisonia	1	2	1	1
Piperaceæ : Peperomia	1	1	1	0
,, Piper	1	0	1	4
Goodenoviaceæ : Scævola	1	4	0	0
Myrsinaceæ : Aegiceras	1	1	0	0
,, Myrsine	1	2	0	1
Sapotaceæ : Achras	1	2	0	1
Jasminaceæ : Jasminum	1	3	0	2
,, Olea	1	1	3	1
Apocynaceæ : Alyxia	2	2	0	2
,, Ochrosia	1	2	0	0
Asclepiadaceæ : Marsdenia	1	5	0	0
Loganaceæ : Geniostoma	1	0	1	0
Convolvulaceæ : Ipomæa	2	3	1	4
Solanaceæ : Solanum	1	13	3	1
Myoporaceæ : Myoporum	1	5	1	1
Verbenaceæ : Premna	1	0	0	0
Labiatæ : Eranthemum	1	1	0	0

Exogens.—*Continued.*	Howe's Island.	N. S. Wales.	New Zealand.	Norfolk Island.
Myrtaceæ: Eugenia	1	4	1	0
„ Leptospermum	1	11	2	0
„ Melaleuca	1	13	0	0
„ Metrosideros	1	0	9	0
Ficoideæ: Mesembryanthemum	1	1	1	1
Umbelliferæ: Apium	1	2	2	0
Araliaceæ: Panax	1	3	6	0
Loranthaceæ: Viscum	1	1	1	1
Rubiaceæ: Coprosma	2	2	19	2
„ Psychotria	1	2	0	0
„ Randia	1	1	0	0
Composit: Cassinia	3	4	3	0
„ Gnaphalium	1	5	9	3
„ Olearia	1	17	6	0
„ Senecio	1	13	18	1
„ Wedelia	1	2	0	1
Epacridaceæ: Dracophyllum	1	1	14	0
„ Leucopogon	1	20	3	0
Leguminosæ: Canavallia	1	1	0	1
„ Dolichos	1	0	0	0
„ Edwardsia	1	0	1	0
„ Guilandina	1	0	1	0
Ranunculaceæ: Clematis	1	3	5	0
Magnoliaceæ: Drimys	2	1	1	0
Menispermaceæ: Stephania	1	1	0	0
Cruciferæ: Capsella	1	0	0	0
„ Lepidium	1	3	2	0
„ Senebiera	1	1	2	0
Pittosporaceæ: Pittosporum	1	4	11	1
Guttiferæ: Calophyllum	1	0	0	0
Malvaceæ: Hibiscus	3	7	1	4
Rutaceæ: Acronychia	2	3	0	0
„ Evodia	1	1	0	1
„ Melicope	1	3	2	0
„ Xanthoxylon	1	1	0	1
Meliaceæ: Dysoxylon	2	4	0	0
Celastrinaceæ: Elæodendron	1	1	0	1
Sapindaceæ: Cupania	1	5	0	0
„ Dodonæa	1	12	7	0
	71.—82.	60.—225.	37.—175.	29.—44.

From these tables it appears that of 17 genera of Cryptogamic plants inhabiting Howe's Island, 14 are found in this colony, 14 in New Zealand, and 9 in Norfolk Island; of 17 genera of Endogenous plants, 14 are again found in this colony, 9 in New Zealand, and only one in Norfolk Island; and of 71 genera of Exogenous plants, 60 are found in this colony, 37 in New Zealand, and 29 in Norfolk Island. There are, therefore, 105 genera on Howe's Island, represented by 135 species; 88 of the same genera are found in this colony, represented by 318 species; 60 of the genera are found in New Zealand, represented by 261

species; while only 39 of the genera are found on Norfolk Island, represented there by 70 species.

This analysis indicates a closer affinity to the plants of this colony, than to those of any of the other places named. The relationship, however, is more in the *genera* than in the *species*, which, as a rule, are widely different. This remark would equally apply to any comparison which might be drawn with those of New Zealand. Taken as a whole, the Flora of the Island is most peculiar and very interesting, agreeing, as has been shewn, in many respects with that of this country, yet presenting a marked distinction in the total absence of *Proteaceæ*, and the extreme rarity of *Leguminosæ*. With New Zealand it is more particularly allied by *Edwardsia*, *Metrosideros*, arborescent *Dracophyllums*, and other genera; with New Caledonia principally by its *palms;* and with Norfolk Island by the frequent occurrence of such plants as *Hibiscus Patersonii*, *Pisonia Brunoniana*, *Xanthoxylon Blackburnia*, *Achras costata*, and the curious little parasite *Viscum opuntioides*, all of which are so largely represented on both islands as to have led me to infer, formerly, that there was a greater connection between the botany of the two places than is really the case.

There are some singular features in the vegetation of this little island, which afford abundance of food for reflection. Whence, it may be asked, came that extraordinary fig tree, the single species of its genus? Whence came those Palms, apparently confined to this one locality? Whence came the settlers' "Wedding Flower," that beautiful iridaceous plant, and the solitary representative of its family, all of which were specially referred to and described in my previous report? And whence came the other plants, of which we had no previous knowledge? Can it be that they were created for this place only? or, were their seeds wafted hither from some unknown country, through oceanic influence? Were they, on the doctrine of chance, generated spontaneously? or, were they brought in embryo by some meteoric stone, as lately suggested might be the case, by an eminent authority? These are all questions which may occur in attempting to reason by what means this island first became clothed with plants of such a novel and interesting character.

As I neither believe in special creation, nor in the agency of the ocean, except in cases such as the cocoa-nut and other similar fruits, which are undoubtedly floated from one land to another, nor in spontaneous generation, nor in the meteoric stone theory, I am constrained to adopt, as the only reasonable solution of the difficulty, that this island, Norfolk Island, New Zealand, New Caledonia, and the islands of the Western Pacific formed at one time either a portion of this or another vast continent.

New Guinea—a highly promising field for settlement and colonization, and how such an object might be most easily and successfully effected.

[Read on the 8th November, 1871.]

New Guinea, with the single exception of this island-continent of Australia, is the largest island on the face of the earth. From its south-eastern to its north-western extremity, it is upwards of thirteen hundred miles in length, its breadth from north to south is from three or four hundred to six or seven hundred miles. It is, fifty thousand square miles larger than France, even including the recently ceded provinces of Alsace and Lorraine, and it is within a hundred miles of the Australian land.

This great island was discovered by the Portuguese in the year 1511, and was revisited by the enterprising navigators of that country in 1527; but Petermann, the eminent German geographer of Saxe Gotha, informs us that it was known and visited for the purposes of trade at a much earlier period by the Arabian traders of the Indian Archipelago; and the great empires which the Portuguese found established in the western parts of the Archipelago, when they commenced their career of ruthless conquest in the east, render this allegation probable enough.

The first Englishman who visited the island, and who, I may add, resided there for some time in Dorg harbour, in latitude fifteen minutes south, on its north-west coast, was a Captain Forrest, of the Honorable East India Company's naval service, a few years before Captain Cook discovered the east coast of Australia, in the year 1770. His object was to search for plants of the nutmeg tree of the Moluccas in New Guinea—an object in which he succeeded—and to carry back with him on his return voyage, a large supply of the plants for the Company's settlement of Palemban, an island on the N. E. of Borneo. Captain Forrest found the natives of a friendly character, and not only disposed but accustomed to trade, in particular with the Malays and the Chinese to the westward; and he pourtrays one of the most remarkable features in their social system, which would assimilate them with the inhabitants of the Lake dwellings in Switzerland and other countries of Europe in the pre-historic ages of man. But as a building of a precisely similar character to the one described by Captain Forrest, on the north-west coast, has

been found within a comparatively recent period, on the banks of a river on the South-east coast, I shall defer any further reference to it for the present. It is principally, however, to the south and south-east coasts of the island that I would direct the attention of the Society during our present meeting.

The first European navigator whoever saw the south-east coast of New Guinea was Luis Vaez de Torres, when running across the Pacific from Mexico or New Spain, to the Molucca Islands, in the year 1606, in the Spanish frigate, *Almiranta*. What De Torres took, however, for "the beginning of New Guinea," as he called it, was merely the eastern extremity of the Louisiade Archipelago, an extensive group of Islands to the South-east of New Guinea. But De Torres subsequently discovered and passed through the Strait that now bears his name, and that separates the mainland of Australia from New Guinea, at Cape York.

The land which De Torres mistook for the east end of New Guinea was afterwards designated by an eminent French navigator, M. Bougainville, in the year 1768, the Louisiade Archipelago, in honour of the then King of France, Louis XV., the *bien aimé*, or well beloved. The existence of the Strait, however, was at that time unknown to European navigators, the Dispatch and Report of Torres to the King of Spain being found untransmitted in the Royal archives at Manilla, when the Philippine Islands were taken and occupied for a time by the English, about a century ago.

Captain Edwards, of H. M. S. *Pandora*, which was wrecked in Torres Straits, when returning to England from Tahiti with the mutineers of the *Bounty* in the year 1791, coasted along a portion of the south-east coast of New Guinea, and named two of its remarkable points Cape Hood and Cape Rodney, in honour of two well-known English admirals. M. D'Urville, another eminent French navigator, coasted along the south-east portion of the coast of New Guinea, in the French discovery ships *L'Astrolabe* and *La Zelee*, in the year 1840.

Captain Blackwood, R. N., of Her Majesty's Surveying ship *Fly*, while pursuing his important labours along the north-east coast of Australia, and the islands in Torres Straits, in the years 1842, 1846, (of which a very interesting account is given by Mr. Jukes, Naturalist to the expedition) examined, more or less closely, about 140 miles of the south-east coast of New Guinea, commencing nearly due north from Cape York, the north-eastern extremity of the Austral land. There is a great bight on this part of the south-east coast of New Guinea, like the Gulf of Carpentaria, on the north, and the great Australian Bight on the south coast of this great South Land, and also like the Bight

of Benin, on the west coast of Africa, the land suddenly trending to the northward for more than two degrees of latitude, and forming a deep gulf to the southward. Along this whole extent of coast for 140 miles, the land is described by Mr. Jukes as follows :—"It was low, flat, muddy, covered with jungle and impenetrable forests, and intersected in every direction by a multitude of fresh water arms and channels, uniting one with the other, and forming a complete network of fresh water canals of all sizes and depths, from a mere muddy ditch to a width of five miles, and a depth of twenty to thirty feet. This coast was fronted by immense mud banks, stretching from ten to twenty miles out to sea, having at low water a general depth of about twelve feet, and a few deeper places, and some sand banks much shallower or quite dry. These mud flats gradually deepened towards their outer edge to three and four fathoms, and then more rapidly to six, ten, fifteen, and twenty fathoms.

Now this is precisely the formation of the delta of a great river, and the only difficulty in the present case, is the supposing a river larger enough to produce such a delta, to exist on an island like New Guinea.

From what we know of the rest of the island, however, the existence of such a river becomes highly probable. A range of high mountainous land runs along all the north coast, from Dampier's Strait to Geelesink Bay. High land also comes out upon the south-west coast, about Triton Bay, where the Dutch once formed a settlement near the 137th meridian. The hollow between these two ranges would run towards the south-east, in which direction, of course, their drainage would be deflected. We have already seen reason to believe that the country is a hot one; and the moisture, which does not fall as rain from the south-east tradewinds as it passes over the flat land, is no doubt caught, and precipitated in abundance on the south-east sides of the mountains, and is thus sent down on to the flat, in the shape of rivers. Whether these ever join into one stream, or whether a number of them all run for the south-east, and thus unite only in forming the delta of which we traversed the outer ledge, is, of course, left open to conjecture. If they ever unite in one stream, it will probably be found to be a very noble one for the size of the island, winding, perhaps, through rich flats of tropical forests. Whatever be the characters of the interior waters, however, they must afford access for small crafts into the very heart of the country. Unlike the rivers of Australia, the estuaries of which are always salt, and the rivers mostly trickling shallow streams, running over rocks or sands, the rivers of New Guinea are so full, and abounding with fresh water, as to influence the sea for miles outside their mouths, and effect the salt water even from

the flattest and most sluggish part of their course. Any craft then, that can get across the mudflats off their mouths, need never fear the being unable to find water enough for many miles above them. No doubt some channels will be much more shoal than others, but a small light steamer, drawing about six feet of water, might probably penetrate for a couple of hundred miles, over into the very heart of the country. We had no means of judging which would be the best channel to take, except that the large southern arm (in lat. 8° 45′) which Captain Blackwood first visited, seemed both the largest and to have the deepest water at its mouth. I know of no part in the world, the exploration of which is so flattering to the imagination, so likely to be fruitful in interesting results, whether to the Naturalist, the Ethnologist, or the Geographer, and, altogether so well calculated to gratify the curiosity of an adventurous explorer as the interior of New Guinea. New Guinea! The very mention of being taken into the interior of New Guinea sounds like being allowed to visit some of the enchanted regions of the Arabian Nights, so dim an atmosphere of obscurity rests at present on the wonders it probably contains."—Narrative of the Surveying Voyage of H. M. S. *Fly*, commanded by Captain F. P. Blackwood, R. N., in Torres Straits, New Guinea, and other Islands of the Eastern Archipelago, during the years, 1842—1846. By J. Beete Jukes, M.A., F.G.S., Naturalist to the Expedition. Vol I., pages 289, 291.

Captain Blackwood ascended the great river which he had discovered on the south-east of New Guinea, and which he named the Aird, from one of his officers, for about 20 miles. It was about two miles broad at that point, and there were numerous villages and a comparatively numerous population on its banks. Every attempt, however, that was made to conciliate, and to hold a parley with the natives, proved unsuccessful, and their persistent efforts to overpower the Expedition by means of arrows shot from their large and numerously manned canoes, rendered it necessary, in the estimation of Captain Blackwood, to resort to force for the defence of his party. In this collision, several of the natives were unfortunately killed, and one of their villages being taken, Captain Blackwood and his party went ashore, to examine the place, and in particular a very large building, which seemed to be the habitation of the entire community, and which Mr. Jukes described as follows:—" The house, or whatever it might be called, was raised from the muddy ground about six feet, resting on a number of posts, placed irregularly underneath it, most of which seemed to be stumps of trees, cut off at that height, and left standing. The floor raised upon these seemed to consist of poles fastened across a framework, on which

were laid loose planks, made apparently of the outer rind of the sago-palm split open, and flattened and dried. This floor was perfectly level and smooth, and felt firm and stable to the feet. It was about thirty feet in width, and upwards of three hundred feet long. Mr. Walsh and I both stepped it from end to end, and I made it 109, and he 110 paces long; both our paces were long ones, and I know my own to be upwards of three feet. The roof was formed of an arched framework of bamboo, covered with an excellent thatch of the leaves of the sago-palm. It was sixteen or eighteen feet high in the centre, from which it sloped down on either hand to the floor. It was perfectly waterproof, as, though it was still raining hard, not a drop came through. The end walls were upright, made of bamboo poles, close together, and at each end were three door-ways, having the form of a gothic arch, the centre being the largest. The inside of the house looked just like a great tunnel. Down each side was a row of cabins: each of these was of a square form, projecting about ten feet, having walls of bamboo reaching from the floor to the roof, and accessible at the side by a small door very neatly made of split bamboo. Inside these cabins we found low frames, covered with mats, apparently bed places, and overhead were shelves and pegs on which were bows and arrows, baskets, stone axes, drums, and other matters. In each cabin was a fireplace (a patch of clay), over which was a small frame of sticks, as before mentioned, about two feet high, three feet long, and a foot wide, as if for hanging something to dry or cook over the fire. A stock of dry firewood was also observed in each cabin on a shelf overhead. One or two of these fireplaces were also scattered about in different parts of the sides of the house. Between each two cabins was a small doorway, about three feet high, closed by a neatly made door or shutter of split bamboo, from which a little ladder gave access to the ground outside the house. At each end of the house was the stage or balcony mentioned before, being merely the open ends of the floor outside the end walls, on which the cross poles were bare or not covered with planks. The roof, however, projected over these stages, both at the sides, and much more overhead, protruding forward at the gable, something like the poke of a lady's bonnet, but more pointed. Inside, all the centre of the house, for about a third of its width, was kept quite clear, forming a noble covered promenade. It was rather dark, as the only light proceeded from the doors at the end, and the little side doors between the cabins. Near the centre, on one side, was a pole reaching from the floor to the roof, on which was a kind of framework covered with skulls;—of these, Dr. Whipple brought away four, two of which he gave Captain Blackwood, who has presented them to the College of Surgeons." *

* Mr. Jukes, *ubi supra* Vol. I., pages 271—274.

Now this very remarkable style of native building, indicative as it is of the social system of New Guinea, is precisely the same as Captain Forrest had observed and described among the natives of Dorg Harbour, on the north-west coast of the great island, a century ago. The only difference is that the building Captain Forrest described in Dorg Harbour was partly built on piles in the sea, into which one of its main entrances opened out into pretty deep water. It was therefore a perfect specimen in actual use of the Lake dwellings of Switzerland and elsewhere in Europe in the pre-historic ages of man. There was a still further difference in the two cases, for whereas the large building above described was appropriated for the married families and the young women and children, there was a separate and smaller building in Dorg Harbour, as a sort of Bachelors' Hall for the unmarried young men of the community. The whole case, so well described by the two navigators, is one of interest in connection with the science of Ethnology, and the enquiries that are now in progress into the condition and habits of certain of the earlier races of men, and it is earnestly to be desired that the subject should be followed up by further investigation in New Guinea.

In connection with this very interesting subject, I may observe, that Mr. Jukes draws a comparison or rather contrast, between the Aborigines of Australia and their congeners of the Papuan race in New Guinea and the islands of the Western Pacific, including the islands in Torres Straits, New Caledonia, the New Hebrides, and the Fiji Islands. "We remained three days," he tells us, "at Evans' Bay [near Cape York] completing our water, where we found a party of five Australians. These men were very quiet and friendly, but contrasted most unfavourably for themselves, with our friends, the Erroobians," the inhabitants of one of the islands in Torres Straits. It was now, indeed, for the first time that I became fully aware of the great difference between the two races, which is both a physical and a mental one. These fine men had the spare thin-legged, lanky build of all the Australian people. Their colour was of a more sooty black than the islanders, who are of a reddish or yellowish brown. The hair of these Australians, however, was like that of the European race, equally diffused, rather fine, and either straight, or commonly waving in broad open curls. Among the islanders the hair invariably grows in tufts or pencils. In their intellectual qualities and dispositions, they were still further removed from the islanders, and much below those of Murray and Darnley islands. Houseless and homeless, without gardens or any kind of cultivation, destitute of the cocoa-nut, the bamboo, the plantain, and the yam, as of almost all useful vegetables, they pass their lives either in the search for food, or in listless indolence. Instead of associating

with us on something like terms of equality, bartering with us, teaching us their words, and learning some of ours, laughing, joking, and engaging in sports like our Erroobian friends, these Australians sat listlessly looking on, standing where we told them, fetching anything, or doing anything we ordered them with great docility indeed, but with complete want of interest and curiosity. In our endeavours to get words from them, they merely repeated our sounds or imitated our gestures. When they spoke, it was difficult to catch the sound, so different was their speech from the clear, open enunciation of the Erroobians. With the latter we often eat, as they were perfectly clean; but these Australians on our shooting a kite or two, instantly seized them, plucked off some of the feathers, and then warming the body a little at the fire, tore it open, and eat it up entrails and all. These Australians at Cape York precisely resembled those of the rest of the continent as I have myself seen them, and as they have been described by other voyagers. The Torres Straits islanders on the contrary evidently belong to the great Papuan race, which extends from Timor and the adjacent islands, through New Guinea, New Ireland, and New Caledonia, to the Fiji Islands.

It is singular enough, that in Torres Strait, the line of demarcation should be almost equally strong and precise between two kinds of vegetation, and two groups of the lower order of animals, as between two varieties of the human race. The dull and sombre vegetation of Australia spreads all over Cape York and the immediately adjacent islands. Wide forests of large but ragged stemmed gum-trees, constitute the characteristic of this vegetation. Here and there are gullies with jungles of more umbrageous foliage, and some palms, but the mass of the woods are arid, hot and dusty, the leaves not only small, but dry and brittle, and the marks of frequent fires everywhere apparent in calcined rocks, and blackened stems and fallen trunks. On the islands of the northern side of Torres Strait, not a gum-tree is to be seen, the woods are close, lofty, and afford the deepest and most refreshing shade, often matted into impenetrable thickets by creepers and undergrowth, but adorned with varied foliage, with the cocoa-nut, the plantain, the bamboo, and other plants, not only beautiful, but useful to man. On the New Guinea coast, the vegetation is of the rankest and most luxuriant character, even for the tropics. One vast dark jungle spreads over its muddy shores, abounding in immense forest trees, whose trunks are hidden by groves of sago-palms, and myriads of other heat and moisture loving plants.

In the vegetable kingdom, a reason for the difference might be sought in the variation of the climate. From the abundance of

fresh water, and from the manners and habits of the people—such as fire wood being stored in the houses, sticks laid across the paths, to keep the passenger from the mud, as well as from our personal experience, while there—the south-east coast of New Guinea, has evidently a very moist climate. It is probable, I think, that during the whole S. E. monsoon, or from the middle of March to the end of October, the weather is rainy, and that during the N. W. monsoon, which brings rain to the north coast of Australia, the south coast of New Guinea may have its dry season. Australia, on the contrary, has a remarkably dry climate, and though there are frequent showers during the S. E. monsoon on the margin of the north-east coast and about Cape York, where the trade wind first strikes upon the land, it is probable that in the interior (as it is certain that on the north coast about Port Essington) no rain falls during the greater part of the year, and heavy showers only during the remainder.

Not only, however, is this variation of climate not sufficient to account for the utter difference in the vegetation of the two countries, Australia and New Guinea, but I much question whether the difference in the climate be not in great part the result of the vegetation. The thick dark woods and jungles of New Guinea, completely protect the soil from the sun, the broad close leaves shelter even the stems of the trees, and all tend to produce a coolness favourable to the precipitation of moisture from the damp trade wind. The open and scattered woodlands of Australia, on the contrary, offer no shelter to the ground from the rays of the sun. The small, thinly disseminated leaves of these evergreen trees, instead of giving shade, become themselves as hot and parched as the rocks and sands beneath them. The ragged strips of dry and resinous bark hanging from the trunks of all the trees, are like tinder, ever ready to catch fire with a spark, and the grass among the trees commonly resembles hay. Everything absorbs the heat freely, and radiates it into the surrounding atmosphere. Instead of being cooled then, and precipitating its superabundant moisture, the sea air on entering an Australian wood, has its temperature raised, and becomes capable of licking up any drop of humidity it may find still lingering there. For this reason, a current of air is seldom perceptible in an Australian forest, which always feels hot, dry, and oppressive. The immediate neighbourhood of Cape York indeed seemed one of the comparatively favoured spots, where frequent showers during the whole year permitted the existence of permanent fresh water pools and green grass during even the driest season.—Mr. Jukes, *ubi supra*, Vol. 1., pages 295, 301.

Captain Blackwood was followed in his survey of the south-east coast of New Guinea, in the years 1846—1850, by the late

Captain Owen Stanley, R.N., in H.M.S. *Rattlesnake.* Captain Stanley took up the work at Cape Possession, on the east side of the Great Bight, where it had been discontinued by Captain Blackwood, and added, including the previous discoveries of Captain Edwards, an extent of about 240 miles to our knowledge of that coast; his own sickness and death before the completion of his voyage imparted an additional and melancholy interest to his valuable labours. This portion of the coast of New Guinea is remarkably different from that surveyed by Captain Blackwood; for whereas the line of coast surveyed by that officer is low, swampy, and covered with dense forests, the whole coast line surveyed by Captain Stanley is backed up, at a distance of from twenty to fifty miles inland, by a noble range of lofty mountains, that give rise to numerous streams, some of which are known to pour down immense volumes of water into the sea.

Of the mountains along this line of coast, the following are those of greatest elevation :—

Mount Yule, 10,046 feet above the level of the sea.
Mount Owen Stanley, 13,205 feet " "
Mount Obree, 10,246 feet " "
Mount Suckling, 11,226 feet " "
Mount Brown, 7,947 feet " "

besides various others of lesser elevation.

In short, there is no country on the face of the earth more admirably fitted for settlement and colonization, or that holds out a higher promise for the future adventurer in that heroic work than the south-east coast of New Guinea. I should not recommend the formation of a settlement on any part of the low line of coast surveyed by Captain Blackwood, as it is not likely, from its physical character, to be salubrious for European life; but the more elevated coast-line surveyed by Captain Stanley, backed up as it is by the lofty mountain of the Owen Stanley Range, affords the highest promise for successful colonization, on the part of adventurers of our own Anglo Saxon race, matured and improved, as they would unquestionably be for such a purpose, by Colonial experience. I should desiderate indeed, above all things in the department of geographical discovery, the fitting out of one or two small steamers to ascend the river which Captain Blackwood discovered in the Great Bight, to the head of its navigation; but I should not like to recommend a settlement for British colonists, on any part of that coast. The coast-line of the Owen Stanley survey, is unquestionably the proper field for British colonization in New Guinea; and it will be a great reproach to our country if it shall not be occupied for that purpose soon. For it is not to be supposed, in the present condition of the great maritime nations of Europe and America,

that if we allow so noble a field for colonization as that line of coast presents to remain much longer unoccupied, it will not be taken possession of for such a purpose by some other maritime power. Prince Bismark, we know, from correspondence with a fellow-countryman and admirer of his own in the Fiji Islands, is on the look-out for a German colony somewhere for the new Empire; and as we have heard recently of a Russian scientific expedition to New Guinea, it is quite possible, and in perfect accordance with the well-known practice of that great annexing power, that the expedition in question may ultimately be found to cover some scheme of future annexation by the Czar. The Owen Stanley coast line is within two hundred and fifty miles of one of our actual settlements, and communication could therefore be kept up with it from thence by means of a small steamer with perfect facility.

The eligibility of this line of coast for the establishment of Christian missions, simultaneously with the carrying out of such plans of colonization as might be deemed practicable and expedient, is too obvious to require a more particular notice. Such missions on the one hand, and such plans of colonization on the other, might not only prove mutually helpful, but would in all likelihood ensure proper treatment for the aborigines on the part of the European adventurers, and shed a flood of light upon many important questions in ethnology. Nothing, for instance, in the whole field of speculation, can possibly be more interesting than the fact that we have almost in our own immediate neighbourhood in this colony a numerous people, dwelling in communities and comparatively somewhat advanced in civilization, in precisely the same social state and circumstances as those of the dwellers in the mysterious habitations of the Lakes of Switzerland before history began. To know something more about these mysterious people would surely be worthy of a great effort on our part.

In regard to the prospect of valuable gold discoveries being likely to be made in that part of New Guinea, the late Mr. John MacGillivray, the naturalist of Captain Owen Stanley's expedition, writes as follows:—

"That gold exists in the western and northern portions of New Guinea has long been known; that it exists also on the south-eastern shores of that great island is equally true, as specimens of pottery procured at Redscar Bay (near Cape Possession on that coast) contained a few small laminar grains of this precious metal. The clay in which the gold is imbedded was probably part of the great alluvial deposit on the banks of the rivers, the mouths of which we saw in the neighbourhood, doubtless originating in the high mountains behind, part of the Owen Stanley Range."*

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* Narrative of the Voyage of H.M.S. *Rattlesnake*, during the years 1845-1850, Vol. II., page 69.

In short, had we, the Britons of the nineteenth century, possessed the same facilities for planting colonies beyond seas as were enjoyed and exercised so ably and so successfully by the ancient Greeks, I am confident that not only New Guinea, but all the important group of islands in the Western Pacific would have been colonized long ago; neither should we have lost Tahiti, or New Caledonia. Unfortunately, however, like the dog in the manger, Great Britain will neither undertake the great work of colonization herself, nor permit it to be undertaken even by her own people. The ancient city of Miletus, a seaport town on the coast of Asia Minor, in all probability no larger than Sydney, had planted not fewer than from eighty to a hundred colonies on the seas that were then travered by the ships of the ancient world. And if we had only the same colonizing power as Miletus, I am confident that we should be equally successful in that heroic work. But we are uniformly stopped at the very first by that law and provision of our constitution that prohibits any British subject from planting a colony in any vacant territory on the face of the earth without the express consent of the Crown. And this, we are all doubtless aware, it is not easy to obtain.

I addressed a letter on this very important subject by the August Mail, to Mr. Cowper, the Agent General of the Colony in London, of which I shall take the liberty to quote the following passage :—

"I am not quite aware what your duties are or will be, in England; but there is one which I conceive you might discharge effectually, not only for the benefit of all these Australian colonies, but for the promotion of the interests of civilization and Christianity throughout the vast Pacific Ocean."

Then, after referring to the case of Fiji, and the Government that has recently been formed in that group of Islands, as a matter of necessity in the first instance, but with such promising results, I proceed as follows :—

"It is discreditable in the highest degree, as well as absolutely suicidal, for Great Britain, the mistress of the sea and the *soidisant* nursing mother and promoter of colonization, to permit the present state of things to subsist any longer in the Pacific. It was simply and solely the expenditure of British money in the founding of this colony that has rendered it practicable for any other Power in the world to plant colonies in the Pacific. And why should we permit any such foreign Power to enter upon our labours, as the French have been permitted to do both in Tahiti and New Caledonia, and virtually to reap where they never sowed. Had we in this colony possessed the requisite power to anticipate their movements, does any person suppose that they would ever have been allowed to set foot in either of these islands?"

We have always a large floating population in all these colonies, who are ready for anything in the way of colonization; and it would be the easiest thing in the world to raise the requisite funds for any feasible scheme of that nature, to be carried out either in New Guinea or in any of the groups of islands farther south. All that is requisite is to give us power to set up a Government of some kind, and to establish our Colonial land laws. From the first I was one of the Provisional Directors of a proposed Company, to form a colony in New Guinea, during the regime of your predecessors, Martin and Parkes. There was no lack at the time of adventurers ready to embark at once,—many of them at their own charges—but on finding that a Government could not be formed for such a community, nor the thing be permitted at all under British law, the proposal fell to the ground.

Could it not be provided, then, either by an Act of the Imperial Parliament, or by the sanction of Her Majesty's Government in any other way, that in the event of any number of British subjects—say not less than one, two, or even three thousand—agreeing to go forth for the settlement of any island, or group of islands, in the Western Pacific; under such terms as might be mutually agreed on beforehand, and approved of by the Government of any British colony, (especially on the condition of their establishing such laws, in regard to the acquisition and proprietorship of land, as are in actual operation in the British colonies) it should be lawful for such a community to set up a government for themselves, either with the consent and co-operation of the native authority, if any such exist, or for themselves solely, if not. We do not require annexation for such a purpose. Let the people govern themselves when they can. If you could only effect such an arrangement as this, it would give a wonderful impulse to colonization in the Southern Pacific, and would greatly benefit this colony."

Mr. John Archibald Campbell, a surgeon well known in Sydney about thirty years since, has, it seems, been proposing to form a Company in London for the colonization of New Guinea, direct from England. This, I conceive, would be a hopeless undertaking; but if any feasible scheme for the colonization of the south-east coast of New Guinea—the portion of the coast being subtended by the Owen Stanley Range of mountains—were proposed in this city, and an assurance given that the adventurers should be permitted by the Imperial authorities to form a government for themselves, there would be no lack either of capital or of men for the carrying of it out in all these Colonies, with every prospect of success. In the earlier days of British colonization, a scheme of this kind was actually carried out in New England in America, for, a Company having been chartered in Plymouth for the plant-

ing of a colony in New England, the Directors simply resolved to transfer themselves and their charter bodily to New England, and the noblest colony or rather series of colonies that were ever planted by man was the result. But the principles of colonization, as Mr. Merivale, one of the highest authorities on the subject, fully admits, were much better understood and much better carried out in the 17th century than in the 19th—under the Stuarts than under Queen Victoria. May we hope, however, that a better state of things in this important respect shall speedily be realized, and that such principles will be recognized and established, with the concurrence of the Imperial Government, as will make this city of Sydney, like the ancient city of Miletus in the flourishing period of Grecian colonization, the mother city of a whole series of flourishing colonies in New Guinea and in the numerous and beautiful islands of the Western Pacific.

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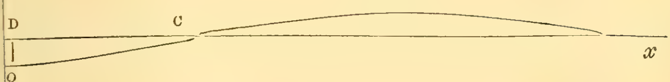
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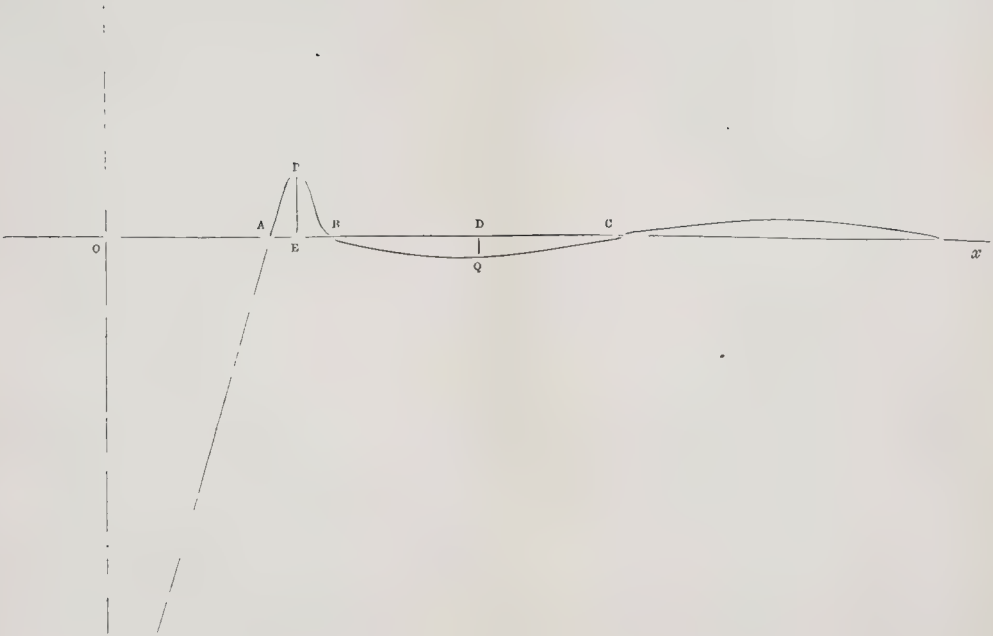
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On the Constitution of Matter, by M. B. Pell, B.A., Professor of Mathematics in the University of Sydney, late Fellow of St. John's College, Cambridge.

[Read, 6th September, 1871.]

In the following paper, an attempt is made to account for some of the properties of matter upon mechanical principles. I assume that solid bodies consist of isolated atoms, whose linear magnitudes are so small compared with the distances between them, that the atoms may be supposed incapable of giving or of receiving any energy except that of translation; and that the mutual action between two atoms is some function of their distance, and acts in the line joining their centres of gravity.

Some writers, whose opinions are entitled to much respect, have expressed an entire want of faith in the theory of isolated atoms acting upon one another at a distance; and some even hold that such a state of things is inconceivable. This is one of those half metaphysical questions upon which perhaps no two men would be found to be exactly agreed; but to me it seems no more difficult to conceive that atoms should have been created with the property of acting upon one another at a distance, than it is to conceive that they should have been created, or have in any way come to exist, or to have any properties at all. It may be that the bodies of the solar system do not act upon one another directly, but they appear to do so, and we are content to assume, provisionally at all events, that they do really so act. There should be no difficulty then in making a similar supposition respecting the atoms of which matter is assumed to consist.

If matter does not consist of isolated atoms but is continuous, any enquiry into its real nature would seem as hopeless as a similar investigation respecting time or space. We could not hope to give any explanation of the facts, for instance, that gold is yellow and soft, and expands under the action of heat, except that it consists of little bits, all of which possess those properties. It will be time to confess that we are reduced to such a method of accounting for the phenomena of nature, when every other has been found to fail.

I have endeavoured to assume as little as possible respecting the mutual action of two atoms, except that it must be such a

function of the distance as to satisfy the most obvious properties of matter. Leaving out of consideration for the moment, all theories except that of gravitation, let us consider what are the facts. Let us consider the mutual action between two atoms or molecules, or particles, or whatever they may be, of a substance such as mercury, which is capable under ordinary circumstances, of existing in the solid, the liquid or the gaseous state. As mercury has weight, we can hardly doubt that at a sufficient distance, two particles of that substance attract one another. At some less distance they repel one another, whatever be the cause, for the vapour of mercury, whether in vacuo, or when mixed with the air, tends to diffuse itself. At a still less distance, in the liquid state, the particles cohere slightly, or attract one another, appearing to be in a relative position of unstable equilibrium. At a slightly diminished distance, the mercury becomes solid, and the attractive force considerable. The solid mercury resists further compression, so that the action again becomes repulsive.

During the last century, Boscovich propounded a theory of alternate attractions and repulsions; but I am not aware of the exact nature of his investigations or speculations, never having had an opportunity of consulting his works. They do not appear to have borne fruit, or to have been received with much favour. It is hardly correct however, to apply the word theory to these attractions and repulsions; we should rather say that they are obvious facts, requiring some theory for their explanation. One theory may be stated thus: the action between two particles is some function of their distance x, which for considerable distances is sensibly equal to $\frac{\mu}{x^2}$, but for very small distances, changes sign several times, becoming finally large and negative, or repulsive. Let us consider whether there is any other tenable hypothesis.

The Dynamical theory of Gases, due chiefly to the labors of Claudius and Maxwell, helps us somewhat. This theory may be considered as established, and as forming the most important addition which has been made to our real knowledge of the laws of inorganic matter in this generation. Maxwell, for reasons assigned, assumes that the gaseous molecules repel one another according to a certain law which makes the force insensible, except at very small distances. The theory of elastic molecules involves a similar assumption; for the elasticity of the molecules must be caused by a repulsive action between their atoms; unless we are to accept an elastic molecule as a finality, beyond which our enquiries into the nature of matter cannot extend. The condensible gases and vapours so closely resemble the permanent

gases in so many of their properties, that it is impossible not to believe that they are governed by the same laws. If the molecules of hydrogen repel one another at certain distances, we cannot doubt that the same is true with respect to chlorine, and carbonic acid gas, and steam. Indeed it would be difficult to know where to draw the line between hydrogen and the most refractory solid. But if the molecules of carbonic acid gas be brought near enough together, they undoubtedly attract, and at still shorter distances again repel one another. If we assume the existence of isolated atoms, there seems no escape from the doctrine of an alternation of actual attractions and repulsions.

Sir Humphry Davy supposed that the repulsive forces between the particles of matter might be of a nature analogous to that which keeps the planets from falling into the sun, or to what commonly goes by the name of centrifugal force. Except that this view of the case is mentioned with approval by a recent writer, I should not have thought it necessary to make use of any arguments to shew that the complicated actions which take place between particles of matter cannot be accounted for by Newton's Law of Attraction alone. That the particles of a solid body should be not only kept apart, but in permanent general relative positions, by centrifugal force alone, seems to me utterly inconceivable, under any known mechanical laws. It may be demonstrated moreover, assuming the theory of atoms, that the cohesive forces of any substance, having any appreciable tenacity, are not only greater, but many millions of times greater, than what would be caused by Newton's Law. It is so far certain then, that that law is not absolutely universal, but is replaced or supplemented by something totally different at very short distances.

The atoms or particles of a solid body certainly seem to be in a position of stable equilibrium, or rather to be vibrating about such a position; and there seems no good reason for doubting that such is, not apparently only, but really the case. The following considerations seem to me to show conclusively, that it cannot be the law of nature that two atoms should attract one another, for all distances however small. It seems natural to suppose that the total quantity of heat, or energy in any solid body, or collection of atoms, is finite, and capable of being expressed as some function of the masses, velocities, mutual actions and relative co-ordinates of the atoms. Now if the atoms attract one another for all distances however small, up to actual contact, then the potential energy, or that due to any relative position of the atoms, would depend partly upon the magnitude and internal constitution of the atoms themselves, and would be, humanly speaking, incapable of definite expression. If the atoms be sup-

posed to be mere points or centres of force, having inertia, but no magnitude; then the potential energy due to the relative position of the atoms of any solid body is infinite. These difficulties disappear if we accept the fact which stares us in the face, that the final action between two atoms, when the distance is diminished, is repulsive; and that for any two atoms of a solid body, there is a relative position of stable equilibrium short of actual contact or coincidence.

Certain considerations in connection with the Dynamical Theory of Gases, require us to suppose that the particles of a gas are not atoms, but molecules, or collections of atoms. The facts revealed by the spectroscope indicate also that the particles of vapours, even in their most attenuated condition, have a complicated constitution, and that they probably consist of a considerable number of atoms. Presuming that the mutual action of atoms is the cause of, and of the same kind as, that which takes place between molecules; that action may be rather rudely represented by the annexed diagram. The abcissa represents the distance between two atoms, and the ordinate the force, the positive ordinate representing attraction. Supposing one atom fixed at O, A is a position of stable equilibrium for the other, and corresponds to the solid state at the absolute zero of temperature. B is a position of unstable equilibrium, and corresponds to the liquid state. Distances greater than OB correspond to the condition of gas or vapour. It is necessary to suppose that in all cases, AB is small compared with OA, and that $\frac{dy}{dx}$ is large at the point A, but generally small at the point B. I do not know that we have any means of forming an opinion as to the relative magnitude of OC, but such a point as C must exist if we assume the transition from Newton's Law to that of gaseous repulsion to be gradual. This necessitates the existence of a maximum negative ordinate DQ. It may be remarked that the distance OD would correspond to the saturation point at the absolute zero of temperature; for supposing a number of equidistant atoms in a confined space, if the distances be intermediate in magnitude between OB and OD, the equilibrium is unstable, but for distances greater than OD, it is stable. For refractory solids EP must be large, and DQ comparatively very small. For gases, AB, or EP, or both, are probably very small, and DQ must be very large, compared with its magnitude for non volatile substances.

If a small velocity be impressed upon the moveable atom in the direction Ox, it will oscillate about A. If the initial velocity be increased to a certain value, that is under the action of a certain amount of heat, the atom will just reach B, and there

remain. With a greater velocity it will pass beyond B, and the solid connection between the atoms will be destroyed. It may be seen without any mathematical investigation that the vibratory motion about A produces two effects.

(1) It increases the mean distance between the atoms; that is, it produces expansion.

(2.) It diminishes the cohesive force between the atoms; that is, it produces softening.

If the initial velocity be sufficient to carry the atom to B, the cohesive force is entirely destroyed, and the condition is that of perfect liquidity; the mean distance between the atoms being then OB. It may be observed that the greater the initial velocity, and the more nearly in consequence the atom approaches B, the position of unstable equilibrium, the greater is the proportion of energy in the potential or latent state. If the atom just reaches B, the whole energy becomes latent.

With reference to the obvious objections that the liquid state, as thus represented, is one of absolute instability, and that the whole of the heat appears to become latent, I must remark in the first place, that probably the liquid condition cannot exist with any permanence except under the combined effects of temperature and pressure; and in the second place, I must anticipate so much as to say, that I hope to succeed in shewing that it is probable that the atoms of a solid, under the action of heat, aggregate themselves into molecules, and assume the liquid and gaseous condition, at a far lower temperature than what could correspond to the velocity necessary to the carry the atom from A to B. That velocity corresponds, not to the melting point of the substance, but to the far higher temperature, higher perhaps than any at present existing in the solar system, under which a molecule would be resolved into atoms.

If x be the distance between the atoms, and $f(x)$ the dynamical measure of the attraction between them, the conditions which have been stated may be approximately expressed by supposing

$$f(x) = (x - \alpha)(\beta - x)^3 \phi(x)$$

where $OA = \alpha$, $OB = \beta$, and $\phi(x)$ is a function which does not change sensibly within the small limits $x = \alpha$, $x = \beta$. Let $\beta - \alpha = h$, $x = \alpha + z$, h and z being supposed small compared with α, then

$$\begin{aligned} f(x) &= z(h - z)^3 \phi(\alpha + z) \\ &= z(h - z)^3 \phi(\alpha) \text{ nearly} \end{aligned}$$

Put $f'(\alpha) = h^3 \phi(\alpha) = m^2$, then

$$f(x) = m^2 z \left(1 - \frac{z}{h}\right)^3$$

It must be observed that this is little, if anything, more than a statement in a mathematical form of some of the most obvious properties of ordinary matter. It remains to be seen whether this statement or assumption is consistent with, and can explain other and more recondite properties.

In order to satisfy approximately the condition that $f'(\beta)$ must be very small, I have made it zero, giving to $f(x) = 0$, three roots equal to β. Any odd number of roots would apparently do as well as 3, but there are good reasons for believing, as I shall shew hereafter, that 3 is the correct index.

The equation of motion is

$$\frac{d^2 z}{dt^2} = -f(x)$$

which may be solved approximately when z is small compared with h. Neglecting the last term the equation may be written—

$$\frac{d^2 z}{dt^2} + m^2 z = \frac{3\,m^2}{h}\left(z^2 - \frac{z^3}{h}\right)$$

When $t = 0$ let $z = a$, $\frac{dz}{dt} = 0$, where $\frac{a}{h}$ is small. For a first approximation we have

$$z = a \cos mt.$$

By the usual method of successive approximations, z may be developed in terms of $\frac{a}{h}$. The process requires some care, but involves no particular difficulty, so that it will be sufficient to give the result, which, to the degree of approximation required for the present purpose, is

$$z = a\,[A \cos cmt + B\,p + C \cos 2cmt + D\,p^2 \cos 3cmt]$$

where $p = \frac{a}{h}$

$$A = 1 - p + \frac{55\,p^2}{32}$$

$$B = \frac{3}{2}\left(1 - 2p + \frac{147\,p^2}{16}\right)$$

$$C = -\frac{1}{2}(1 - 2p)$$

$$D = \frac{9}{32}$$

$$c^2 = 1 - \frac{21\,p^2}{4}$$

c being a factor nearly equal to unity, which, as in the Lunar Theory, it is necessary to introduce. In conducting the

approximations the condition $z = a$ when $t = 0$ is preserved throughout. The last term is not made use of in the final application of the result, but is required in the course of the approximations.

The temperature may be assumed to be proportional to the average vis viva, or to

$$\frac{\int_0^{\frac{2\pi}{cm}} \left(\frac{dz}{dt}\right)^2 dt}{\frac{2\pi}{cm}} = \frac{a^2c^2m^2}{2}(A^2 + 4p^2C^2)$$

which, neglecting higher powers of p

$$= \frac{m^2h^2p^2}{2}\left(1 - 2p + \frac{3p^2}{16}\right)$$

This is proportional to τ, the absolute temperature, so that we may put

$$p^2\left(1 - 2p + \frac{3p^2}{16}\right) = \lambda\tau$$

where λ is some constant. If M be the mass of an atom, H the quantity of heat to each atom, J the mechanical equivalent of heat,

$$JgH = \frac{\mathrm{M}u^2}{2}$$

where u is the velocity when $z = 0$. It may be easily shewn that

$$u^2 = m^2h^2p^2\left(1 - 2p + \frac{3p^2}{2}\right)$$

$$H = \frac{\mathrm{M}\,m^2h^2p^2}{2Jg}\left(1 - 2p + \frac{3p^2}{2}\right)$$

and approximately,

$$p^2\left(1 - 2p + \frac{3p^2}{2}\right) = \lambda\tau + \frac{21\lambda^2\tau^2}{16}$$

$$\therefore H = \frac{\mathrm{M}\,m^2h^2\lambda\tau}{2Jg}\left(1 + \frac{21\lambda\tau}{16}\right)$$

If s be the specific heat at the temperature τ, or the quantity of heat necessary to raise a unit of mass through one degree,

$$s = \delta\left(\frac{\mathrm{H}}{\mathrm{M}}\right) = \frac{m^2h^2\lambda}{2Jg}\left(1 + \frac{21\lambda\tau}{8}\right)$$

If σ be the specific heat at the absolute zero,

$$s = \sigma \left(1 + \frac{21 \lambda \tau}{8}\right) = \sigma \left(1 + \varepsilon \tau\right)$$

Where $\lambda = \dfrac{2 J g \sigma}{m^2 h^2}$ $\quad \varepsilon = \dfrac{21 J g \sigma}{4 m^2 h^2}$

The expansion is

$$\frac{\int_0^{\frac{2\pi}{mc}} z \, dt}{\frac{2\pi}{mc}} = B a p$$

And the rate of expansion

$$\frac{B a p}{\alpha} = \frac{3 p^2 h}{2 \alpha} \left(1 - 2p + \frac{147 p^2}{16}\right)$$

$$= \frac{3 \lambda h \tau}{\alpha} \left(1 + 9 \lambda \tau\right)$$

$$= e \tau \left(1 + e_1 \tau\right)$$

which is in accordance with the known laws of expansion, e and e_1 being constants.

Suppose a solid body consisting of n equal atoms, and let $x\ y\ z$ represent a very small displacement of an atom. There will be $3n$ linear differential equations for the determination of such quantities as $x\ y\ z$; and it may be shewn that

$$x = \Sigma\, a \cos\left(\mu t + l\right),\ y = \Sigma\, b \cos\left(\mu t + l\right),\ z = \Sigma\, c \cos\left(\mu t + l\right)$$

where μ and l have $3n$ different values which are the same for all quantities, such as $x\ y\ z$. The displacement being small, and no change in the constitution of the body being supposed to take place, terms involving e^t or e^{-t} cannot, from the nature of the case occur. The whole heat for this atom is proportional to

$$\tfrac{1}{2} \Sigma \mu^2 \left(a^2 + b^2 + c^2\right);$$

and the heat developed as temperature is proportional to one-half the non-periodic terms in

$$\left(\frac{dx}{dt}\right)^2 + \left(\frac{dy}{dt}\right)^2 + \left(\frac{dz}{dt}\right)^2$$

$$= \tfrac{1}{4} \Sigma \mu^2 \left(a^2 + b^2 + c^2\right)$$

It follows therefore, that for every atom, at very small temperatures, one half the heat is developed as temperature, and the remainder is latent. If then, there be two bodies, the masses of whose atoms are M and M_1 respectively; at the same small tem-

perature, the whole heat per atom will be the same for both; and if σ and σ_1 be their specific heats at the absolute zero of temperature, we have

$$M \sigma = M_1 \sigma_1$$

which acords with what is called the constancy of the atomic heat of simple substances in the solid state. For such substances we should have $M\sigma = \varkappa$, where $\varkappa$ is constant. If it were possible for two atoms M and M_1 to become united into a single atom, and s were the absolute specific heat of the compound, we should have $(M + M_1) s = \varkappa$. But when two equivalents are chemically combined it is found that $(M + M_1) s = 2\varkappa$; and if there be p of one and q of the other

$$(pM + qM_1) s = (p + q) \varkappa$$

This is what might be inferred from the above considerations, for $\frac{pM + qM_1}{p + q}$ is the average mass of an atom of the compound, and $\frac{pM + qM_1}{p + q} s$, the average heat per atom to produce a rise of 1° from the absolute zero, and therefore equal to $\varkappa$.

This subject is very fully treated in a valuable memoir by Kopp in the Philosophical Transactions. He points out that the circumstance that $\varkappa$ is nearly the same for most simple solids, does not indicate necessarily that they are really simple, but that they are of the same order of composition. There is some difficulty in the theory however, for Kopp remarks, that the known change of specific heat with change of temperature is not sufficient to account for the observed differences in the values of $\varkappa$, even for those substances which nearly satisfy the law. This difficulty disappears, I think, when we observe that the quantity estimated and recorded as the atomic heat is

$$M \sigma (1 + \varepsilon\tau),$$

and although ε is very small, it is different for different substances, and τ being the absolute temperature is considerable. If we had the means of reducing the observations with certainty to the absolute zero, it is probable that the discrepancies would disappear.

The most general case which I have yet attempted to investigate in connection with the motion of atoms, is that of n atoms in a straight line. This is far, of course, from being an arrangement which the atoms of a molecule would really assume and maintain; but it is a theoretically possible combination, and having some generality, its consideration may enable us to form by analogy, an idea of the nature of molecular arrangements, and lead the way to something better.

I undertook the investigation originally, for the purpose of determining the laws of expansion and change of specific heat, but I have been led to conclusions having reference to phenomena of far greater interest and importance.

The law of force, which I have assumed, affords a reasonable general explanation of some of the phenomena relating to gases and vapours; such as the change of specific heat with change of temperature, and condensation at the dew point. It points also to an essential distinction between gases and vapours in the nature of the encounters between the molecules. I must, however, defer the consideration of these and many other questions to some future occasion, briefly stating the principle upon which the change of specific heat, of which the absorption of heat in liquefaction and in vaporization are particular cases, seems to depend. If any system, not subject to loss of energy by friction or any similar cause, be vibrating about a position of stable equilibrium, the average proportion of the energy in a potential state, depends greatly upon whether or not there is any position of unstable equilibrium within, or nearly within, the scope of the motion. When the system passes slowly through such a position, a large proportion of the energy becomes potential; and if the motion constitutes heat, a large proportion of the heat becomes latent. The existence of the position of unstable equilibrium B, appears to be the chief cause of the various changes which are observed in the specific heats of solids, gases and vapours.

Let there be n equal atoms in a straight line, acting upon one another according to the law already assumed, and slightly disturbed in that line from their positions of stable equilibrium. Let the displacements at time t, be represented by $x_1\ x_2\ \ldots\ldots$ $x_r\ \ldots\ldots\ldots$ The equations of motion, to a first approximation, are

$$\frac{1}{m^2}\ \frac{d^2 x_1}{dt^2} + x_1 = x_2$$

$$\frac{1}{m^2}\ \frac{d^2 x_2}{dt^2} + 2\,x_2 = x_1 + x_3$$

$$\cdots\cdots\cdots \qquad \cdots\cdots\cdots$$

or putting q for $\frac{1}{m^2}\left(\frac{d}{dt}\right)^2 + 2$,

$$\left.\begin{aligned}(q-1)\,x_1 &= x_2\\ q\,x_2 &= x_1 + x_3\\ \cdots\cdots\cdots &\quad \cdots\cdots\cdots\\ q\,x_r &= x_{r-1} + x_{r+1}\\ \cdots\cdots\cdots &\quad \cdots\cdots\cdots\\ (q-1)\,x_n &= x_{n-1}\end{aligned}\right\} \cdots\cdots\cdots(1)$$

Let these equations be multiplied by $f_1\, f_2 \ldots\ldots f_r \ldots\ldots\ldots$ respectively and added together; and let all the terms disappear from the resulting equation, except that involving x_n. We must have then

$$q\, f_r = f_{r-1} + f_{r+1}$$

This gives us

$$f_r = A \cos\,(r\,\theta + B\,)$$

where

$$q = 2 \cos \theta$$

The condition that x_1 disappears is

$$(2 \cos \theta - 1)\, f_1 = f_2$$

whence $B = -\frac{1}{2}\theta$; and supposing $f_1 = 1$, we have

$$f_r = \frac{\cos\,(r - \frac{1}{2})\theta}{\cos \frac{1}{2}\theta}$$

The corfficient of x_n in the resulting equation is

$$(2 \cos \theta - 1)\, f_n - f_{n-1} = - \frac{2 \sin n\,\theta \sin \frac{1}{2}\theta}{\cos \frac{1}{2}\theta}$$

and the equation is

$$- \frac{2 \sin n\,\theta \sin \frac{1}{2}\theta}{\cos \frac{1}{2}\theta}\, x_n = 0$$

In a similar manner we obtain

$$+ \frac{2 \sin n\,\theta \sin \frac{1}{2}\theta}{\cos \frac{1}{2}\theta}\, x_1 = 0$$

and since

$$2 \cos \theta.\; x_r = x_{r-1} + x_{r+1}$$

$$x_r = A \cos\,(r\,\theta + B)$$

The first equation gives $B = -\frac{1}{2}\theta$, and therefore

$$x_r = A \cos\,(r - \tfrac{1}{2})\,\theta$$

$$x_1 = A \cos \tfrac{1}{2}\theta$$

$$x_r = \frac{\cos\,(r - \frac{1}{2})\,\theta}{\cos \frac{1}{2}\theta}\, x_1$$

If we put $\dfrac{\pi}{2n} = \gamma$, we have

$\sin n\,\theta = \sin \theta\,(2 \cos \theta - 2 \cos 2\,\gamma)\;(2 \cos \theta - 2 \cos 4\,\gamma)$
$\ldots\ldots\ldots(2 \cos \theta - 2 \cos 2\,(n - 1)\,\gamma)$

$\dfrac{2 \sin n\,\theta \sin \frac{1}{2}\theta}{\cos \frac{1}{2}\theta} = 4 \sin^2 \tfrac{1}{2}\theta\;(- 4 \sin^2 \tfrac{1}{2}\theta + 4 \sin^2 \gamma)\;(-4 \sin^2 \tfrac{1}{2}\theta$
$+ 4 \sin^2 2\,\gamma)\ldots\ldots\ldots\ldots$

And since $2\cos\theta = \frac{1}{m^2}\left(\frac{d}{dt}\right)^2 + 2$, $-4\sin^2\frac{1}{2}\theta = \frac{1}{m^2}\left(\frac{d}{dt}\right)^2$,

the equation for determining x_1 becomes

$$\frac{d^2}{dt^2}\left(\frac{d^2}{dt^2} + \mu_1^2\right)\ldots\ldots\left(\frac{d^2}{dt^2} + \mu_s^2\right)\ldots\ldots\left(\frac{d^2}{dt^2} + \mu_{n-1}^2\right) x_1 = 0 \ldots\ldots\ldots\ldots (2)$$

where $\mu_s = 2\,m\sin s\gamma$, s being any number from 1 to $n - 1$.

If $\frac{d x_r}{dt} = 0$ when $t = 0$, for all values of r, we have

$$x_1 = a + a_1\cos\mu_1 t + \ldots + a_s\cos\mu_s t + \ldots\ldots a_{n-1}\cos\mu_{n-1} t$$

Where $a\ a_1 \ldots\ldots a_s$ are arbitrary constants. It may be easily shewn that a is the same for all values of r, and since

$$x_r = \frac{\cos\left(r - \frac{1}{2}\right)\theta}{\cos\frac{1}{2}\theta}\, x_1$$

and that, when operating upon $\cos\mu_s t$, $\theta = 2\,s\gamma$

$$x_r = a + \sum_{s=1}^{s=n-1} a_s \frac{\cos(2\,r - 1)\,s\gamma}{\cos s\gamma}\cos\mu_s t \ldots\ldots (3)$$

If $x_r = 0$ when $t = 0$, for all values of r, we find in the same way

$$x_r = b\,t + \sum_{s=1}^{s=n-1} b_s \frac{\cos(2\,r - 1)\,s\gamma}{\cos s\gamma}\sin\mu_s t \ldots\ldots (4)$$

r being the number of the atom, s the number of the term in x_r and $b\ b_1 \ldots\ldots$ arbitrary constants.

Suppose the initial conditions to be

$$x_r = \phi(r),\quad \frac{dx_r}{dt} = 0$$

where ϕ is of any given form. For the determination of the arbitrary constants, we have n equations of the form

$$\phi(r) = a + \sum_{s=1}^{s=n-1} a_s \frac{\cos(2\,r - 1)\,s\gamma}{\cos s\gamma} \ldots\ldots (5)$$

It may be shewn that if p and q be any two integers

$$\sum_{r=1}^{r=n} \cos(2\,r - 1)\,p\gamma\ \cos(2\,r - 1)\,q\gamma = 0$$

except when $p = q$, when the sum is $\frac{n}{2}$. If then the equations of the form (5) be multiplied respectively by $\cos s\gamma$, $\cos 3\,s\gamma \ldots\ldots$

$\cos(2r-1)s\gamma$ ...... and added together, all the terms on the right hand side disappear, except those involving a_s, and we have

$$\sum_{r=1}^{r=n} \phi(r) \cos(2r-1)s\gamma = \frac{n\,a_s}{2\cos s\gamma}$$

and adding together equations (5) as they stand

$$\Sigma\,\phi(r) = n\,a$$

$$\therefore\ x_r = \frac{1}{n}\Sigma\,\phi(r) + \frac{2}{n}\sum_{s=1}^{s=n-1}\sum_{r=1}^{r=n}(\phi(r)\cos(2r-1)s\gamma)$$
$$\cos(2r-1)s\gamma\cos\mu_s t\ldots\ldots\ldots(6)$$

If the initial conditions be, $x_r = 0$, $\frac{dx_r}{dt} = \phi_1(r)$, then from equation (4)

$$\phi_1(r) = b + \Sigma\,b_s\,\mu_s\,\frac{\cos(2r-1)s\gamma}{\cos s\gamma}$$

These equations give as before

$$\sum_{r=1}^{r=n}\phi_1(r)\cos(2r-1)s\gamma = \frac{n\,\mu_s b_s}{2\cos s\gamma}$$

$$\Sigma\,\phi_1(r) = n\,b$$

$$\therefore\ x_r = \frac{1}{n}\Sigma\,\phi_1(r)\,t + \frac{2}{n}\sum_{s=1}^{s=n-1}\Sigma\Big(\phi_1(r)\cos(2r-1)s\gamma\Big)$$
$$\frac{\cos(2r-1)s\gamma}{\mu_s}\sin\mu_s t\ \ldots\ldots\ldots\ (7)$$

which completes the solution to a first approximation.

It may be observed that the formulæ given by Poisson for the longitudinal vibrations of an elastic rod may be easily deduced from the above results. It is remarkable also, that by putting $t = o$, and $x_r = \phi(r)$ in equation (6), a general analytical theorem may be deduced, of which Lagrange's theorem, that when $\phi(0) = 0$ and $\phi(a) = 0$

$$\phi(x) = \frac{2}{a}\sum_0^\infty\left(\int_0^a\phi(x)\sin\frac{n\pi x}{a}\right)\sin\frac{n\pi x}{a}$$

is a particular case.

In order to determine how the system would vibrate if disturbed, and then left to itself; suppose the first atom to receive a blow impressing upon it a velocity $m\,a$, which if the next atom were fixed, would cause it to vibrate through a space a nearly, a being small compared with h. We have then $\phi(r) = 0$, $\phi_1(r) = ma$ when $r = 1$, and zero for all other values.

$$\Sigma\,\phi_1(r)\cos(2r-1)s\gamma = m\,a\sin s\gamma$$

$$\Sigma\,\phi_1(r) = m\,a$$

$$x_r = \frac{m\,a\,t}{n} + \frac{a}{n}\sum_{s=1}^{s=n-1}\sin(2r-1)s\gamma\sin\mu_s t$$

The nature of the vibrations will be the same, and the term $\frac{m\,a\,t}{n}$, denoting the general motion of translation of the system, be avoided, by supposing the initial conditions to be

$$\phi\,(r) = \frac{a}{n}\ \Sigma \sin\ (2\,r - 1)\ s\,\gamma \quad , \quad \phi_1\ (r) = 0$$

$$\text{then } x_r = \frac{a}{n}\ \sum_{s=1}^{s=n-1} \sin\ (2\,r - 1)\ s\,\gamma \cos \mu_s\, t$$

Since $2\,n\pi = \gamma$, it is evident that

$$x_{n-r+1} = -\,x_r$$

so that the motion is symmetrical about the middle point. If n be an odd number the central atom will remain at rest. These conclusions hold for the higher approximations as well as for the first.

The whole energy is $\frac{m^2\,a^2}{2}$, and the vibratory energy, which alone is represented in the above value of x_r, is $\frac{n-1}{2\,n}\ m^2 a^2$. The heat developed as temperature for the r^{th} atom is

$$\frac{m^2\,a^2}{n^2}\ \Sigma \sin^2 s\,\gamma \sin^2\ (2\,r - 1)\ s\,\gamma = \frac{m^2\,a^2\ (n - 1)}{4\,n^2}$$

and is the same for all, the mass of an atom being here supposed to be unity.

Subject to no external disturbance whatever, any number of atoms might vibrate together in the manner indicated, but this is a condition which can never exist, for the atoms of a molecule of vapour, even during the interval between its encounters, are subject to acceleration or retardation, as the case may be, from the action of the ether in which they must be supposed to be immersed. A notion may be formed of the effect of an external disturbance upon such a system as that under consideration, by supposing an additional atom at the beginning of the series, constrained to move according to a particular law. Let x_0 be the displacement of this atom, and suppose

$$x_0 = a \cos \mu t$$

a being small compared with h. The equations of motion are

$$q\,x_1 = x_0 + x_2$$

....

$$q\,x_r = x_{r-1} + x_{r+1}$$

....

$$(q - 1)\ x_n = x_{n-1}$$

where as before $q = 2 + \frac{1}{m^2}\left(\frac{d}{dt}\right)^2 = 2\cos\theta$

If these equations be multiplied by $f_1\ f_2$ .... respectively, and added together, and all the terms disappear from the resulting equation, except those involving x_0 and x_n, we have

$$2\cos\theta\ f_r = f_{r-1} + f_{r+1}$$

which gives $f_r = A\sin(r\,\theta + B)$.

The condition $2 f_1 \cos\theta = f_2$ gives $B = 0$, so supposing $f_1 = 1$, we have

$$f_r = \frac{\sin r\,\theta}{\sin\theta}$$

and the corfficient of x_n is

$$(2\cos\theta - 1) f_n - f_{n-1} = \frac{\cos(n + \frac{1}{2})\,\theta}{\cos\frac{1}{2}\,\theta}$$

The equation is therefore

$$\frac{\cos(n + \frac{1}{2})\,\theta}{\cos\frac{1}{2}\,\theta}\ x_n = a\cos\mu\,t$$

We have also

$$x_r = A\cos(r\,\theta + B)$$

and the condition $(2\cos\theta - 1)\ x_n = x_{n-1}$ gives $B = \frac{2n + 1}{2}\,\theta$, so that

$$x_r = A\cos(n - r + \tfrac{1}{2})\,\theta$$
$$x_n = A\cos\tfrac{1}{2}\,\theta$$
$$x_r = \frac{\cos(n - r + \frac{1}{2})\,\theta}{\cos\frac{1}{2}\,\theta}\ x_n$$
$$= a\ \frac{\cos(n - r + \frac{1}{2})\,\theta}{\cos(n + \frac{1}{2})\,\theta}\ \cos\mu t$$

If we put $\mu = 2m\sin\psi$, then, when operating upon $\cos\mu t$, $\theta = 2\,\psi$, and

$$x_r = a\ \frac{\cos(2n - 2r + 1)\,\psi}{\cos(2n + 1)\,\psi}\ \cos\mu t$$

The tendency to rupture is a function, not of the displacements, but of the relative displacements of the atoms, represented by

$$x_r - x_{r-1} = \frac{2a\sin(2n - 2r + 2)\,\psi\sin\psi}{\cos(2n + 1)\,\psi}\ \cos\mu t$$

If n be considerable, but ψ so small that $(2n+1)\psi$ is not nearly equal to $\frac{\pi}{2}$, $\sin\psi$ will be small, and the relative disturbance small compared with a for all values of r. This shews that a slow external disturbance, corresponding to a small value of μ, will cause a general oscillatory motion of the whole system, but very little internal relative vibration of the atoms.

If $(2n+1)\psi$ be equal to $\frac{\pi}{2}$, or to any odd multiple of it, $x_r - x_{r-1}$ becomes infinite, indicating the well known change in the form of the solution from $A\cos\mu t$ to $At\sin\mu t$. I will defer the consideration of this particular case, and suppose $(2n+1)\psi$ to be nearly equal to some odd multiple of $\frac{\pi}{2}$, so that $\cos(2n+1)\psi$ is small, but a so small that the character of the vibrations is maintained. Since

$$\operatorname{Sin}(2n-2r+2)\psi = \cos(2r-1)\psi$$

nearly, the relative displacement is a maximum and large compared with a for such values of r as make $(2r-1)\psi$ a multiple of π or most nearly so; and this of course occurs at regular intervals. If p be the whole number most nearly satisfying the condition $2p\psi = \pi$, the atoms are arranged in groups of p each, where p is a number depending upon the wave length of the disturbance, and the nature of the system, and not at all upon a, or the intensity of the disturbance. Suppose now a, or the temperature to increase gradually; the groups remain the same, but become more and more isolated. Each group acquires an oscillatory motion as a whole in addition to the vibratory motion of its atoms amongst themselves; and the time of this oscillation corresponds nearly to the fundamental or lowest note of a group of p atoms vibrating without restraint, for in that case

$$\mu_1 = 2m\sin\frac{2\pi}{p} = 2m\sin\psi = \mu \text{ nearly}$$

The disturbance $a\cos\mu t$ representing the prevailing heat, μ, and therefore ψ also, for atoms of a given kind, has a certain very limited range of value. We may suppose then that there is some one particular value of ψ making $\frac{\pi}{2\psi} = p$ an integer; and that particular wave length corresponds exactly to the fundamental note of the group, or molecule, of p atoms.

If we suppose now that a increases until the maximum value of the relative displacement exceeds a certain quantity, the severance becomes complete. This does not necessarily take place at all

points at once, for the weakening of the connection between the groups would impede the propagation of the disturbance. The first group would first melt off; or if the temperature were higher, would fly off as a molecule of vapour; and the next group would then be directly exposed to the disturbance, and be melted or evaporated in its turn. It should be observed that the energy employed in severing the connection between the groups, does not increase the temperature, but becomes latent. I must remark here that the liquid condition would be better accounted for, and some other phenomena perhaps explained, by supposing the three roots equal to β of the equation $f(x) = 0$, to be replaced by three roots nearly equal to each other; that is roots whose differences are generally small compared with h. If it be objected that I am assuming an unnaturally complicated and fantastic law, I can only repeat that it is not assumed arbitrarily, but is little more than a reflex of plain facts. If all the various phenomena relating to inorganic matter, are to be accounted for by the motions of atoms acting upon one another according to some one law; and this assumption must be the foundation of every such attempt as the present; it is hardly reasonable to suppose that the law of action which is to be the cause of such a vast variety of complex relations, should be of a very simple kind.

Having considered the motion of the system whilst being heated as it were, let us now consider what would occur, if the disturbance were to cease, and the system be left to vibrate of itself. When $\mu\, t$ is any multiple of $2\,\pi$

$$x_r = a\,\frac{\cos\,(2\,n - 2\,r + 1)\,\psi}{\cos\,(2\,n + 1)\,\psi},\quad \frac{d\,x_r}{d\,t} = 0$$

At any such instant let the disturbing atom be removed, and we have for the subsequent motion, the initial conditions

$$\phi\,(r) = a\,\frac{\cos\,(2\,n - 2\,r + 1)\,\psi}{\cos\,(2\,n + 1)\,\psi},\quad \phi_1\,(r) = 0$$

We suppose that the system is an aggregation of molecules, formed as above described, under the action of the prevailing heat; so that n is a multiple $p\,v$ of p, and if $2\,p\,\psi = \pi$ exactly, we have

$$2\,n\,\psi = v\pi,\ \phi\,(r) = a\,\frac{\cos\,(2\,r - 1)\,\frac{\pi}{2p}}{\cos\,\frac{\pi}{2p}} = a\,\frac{\cos\,(2\,r - 1)\,v\,\gamma}{\cos\,v\,\gamma}$$

Referring to equation (6), $\Sigma\,\phi\,(r) = 0$

$$\Sigma_{r=1}^{r=n}\,\phi\,(r)\,\cos\,(2\,r - 1\;s\,\gamma$$

$$= \frac{a}{\cos\,v\,\gamma}\,\Sigma\cos\,(2\,r - 1)\,v\,\gamma\cos\,(2\,r - 1)\,s\,\gamma$$

which vanishes for all values of s, except $s = v$, and in that case is $\dfrac{a\, n}{2 \cos v\, \gamma}$

$$x_r = a \cos (2\, r - 1)\, v\, \gamma \cos \mu_v\, t$$
$$\mu_v = 2\, m \sin v\, \gamma = 2\, m \sin \psi = \mu$$
$$x_r = a \frac{\cos (2\, r - 1)\, \psi}{\cos \psi} \cos \mu\, t$$

or the motion continues, in this case, to be the same exactly as that impressed upon the system by the disturbance. The system in cooling would radiate the same kind of heat as that which it received; that is, it would give back the fundamental note of its molecules quite pure.

If it shall hereafter appear that we are justified in inferring that atoms under any natural arrangement, under the action of heat of a certain wave length, would behave in a manner analogous to that which they appear to adopt when constrained to move in a straight line, then I think it will be found that we have fallen upon a principle of great importance in the economy of nature. It may be briefly stated thus. The arrangement of atoms in a molecule is caused by the prevalent heat, and depends upon its wave length; and every molecule generated under the action of heat of a certain wave length, radiates heat of the same, or nearly the same, wave length. I do not consider of course, that the existence of this principle is proved, or that these investigations afford us anything more perhaps, than a hint of the truth. In the further remarks which I shall have to make, I shall however assume the truth of the principle which I have stated; but I hope that it will be understood, if I appear to adopt too confident a tone, that I do so merely to avoid the awkward recurrence of a hypothetical mode of expression.

Before indulging in any speculations, I must dwell a little longer upon the dry forms. We will endeavour to form some general idea of the value of ψ from the equations $\mu = 2\, m \sin \psi$, $2\, p\, \psi = \pi$. μ, we know, is very large, and m for ordinary solids must also be very large, for the molecular forces are enormous in relation to the masses upon which they act. If $\psi = \frac{\pi}{4}$, then $p = 2$; if ψ is greater than $\frac{\pi}{3}$, $p = 1$, or the supposed molecular arrangement would not occur. If there be any simple substance for which ψ is greater than $\frac{\pi}{3}$ it must exist in the state of independent atoms and be incapable of assuming the liquid state,

except perhaps under great pressure. If heated it would pass at once from the solid to the vapourous state. It would shew no bright lines in the spectrum at any temperature. In a state of vapour no heat could be consumed in internal vibrations, so that Maxwell's factor β would in this case be unity. I am not aware that there is any substance possessing these properties, but at all events we may presume from the complicated constitutions which molecules appear to possess, that p is generally considerable and ψ therefore small. The above equations give us

$$\frac{\delta p}{p} = - \frac{\delta \psi}{\psi} = - \frac{\delta \mu}{\mu \psi \cot \psi}$$

$\delta \mu$ represents the range of μ in the prevalent heat, and for heat of considerable intensity, $\frac{\delta \mu}{\mu}$ is small, and ψ being small $\frac{\delta \psi}{\psi}$ does not differ much from $\frac{\delta \psi}{\mu}$. If for any substance ψ were small enough, δp might exceed unity, even within the range of heat of considerable intensity, in which case there would be different values of p for different wave lengths. The molecular arrangements of such a substance would display great instability. A molecule formed under one wave length would be decomposed, and otherwise arranged by heat of a different kind. As we have every reason to believe that this kind of instability does not exist in the case of simple substances, we may infer that ψ is not very small: the general conclusion being that for ordinary substances ψ is small but not very small.

The particular value of ψ which for any substance is equal to $\frac{\pi}{2p}$ is not the one which produces the greatest effect in arranging the atoms into groups. The greatest effect is caused by the value of ψ which makes $(2n + 1)\psi$ equal to an odd multiple of $\frac{\pi}{2}$; and when n is large, such a value must exist. In this case the expression for the relative displacement becomes infinite. This does not indicate that there would necessarily be a rupture of the system, but merely that the displacement cannot be expressed exactly in the manner supposed. The best way of stating the case is that a vibration of that exact wave length cannot be propagated through the system at all. By the effect of the factor c, by which in the second approximation, it becomes necessary to multiply μ, the time of vibration and the wave length are increased, so that if ψ be continuous, this particular

vibration would be assimilated to, and fall in with that corresponding to some smaller value of ψ. As a increases, this effect increases and extends to values of ψ nearly equal, within a certain range, to the particular value in question.

The temperature of the r^{th} atom in the case under consideration is

$$\frac{\mu^2 a^2 \lambda}{4} \frac{\cos^2 (2n - 2r + 1) \psi}{\cos^2 (2n + 1) \psi}$$

λ being some constant; and the average temperature is

$$\frac{\mu^2 a^2 \lambda}{4n} \Sigma \frac{\cos^2 (2n - 2r + 1) \psi}{\cos^2 (2n + 1) \psi} = \frac{\mu^2 a^2 \lambda}{8 \cos^2 (2n + 1) \psi}$$

nearly when n is large. The maximum temperature is

$$\frac{\mu^2 a^2 \lambda}{4 \cos^2 (2n + 1) \psi}.$$

There is thus a concentration of heat at the joints, so that at those points a greater softening takes place than would occur if the heat were uniformly distributed. This effect is increased as the temperature increases, on account of the increasing proportion of the heat, employed in softening the joints, which becomes latent. The temperature therefore at which melting occurs is much lower than what, if uniformly distributed, would dissever the atoms.

The effect of radiant heat upon a system of n atoms, such as that under consideration, may perhaps be represented by supposing the additional atom at the beginning of the series to be of feeble power, proportional to λm^2, when λ is very small. Let x_0 be the displacement of this atom, and

$$x_0 = a \cos \mu t$$

The equation of motion for the first of the n atoms is

$$\frac{d^2 x_1}{dt^2} = m^2 (x_2 - x_1) + \lambda m^2 (x_0 - x_1)$$

or $\frac{1}{m^2} \frac{d^2 x_1}{dt^2} + (1 + \lambda) x_1 = \lambda x_0 + x_2$

neglecting λ in comparison with 1, and putting

$\frac{1}{m^2} \left(\frac{d}{dt}\right)^2 + 2 = q = 2 \cos \theta$, we have

$$(q - 1) x_1 = x_2 + \lambda x_0$$
$$q x_2 = x_1 + x_3$$
$$\cdots\cdots\cdots \quad \cdots\cdots\cdots$$
$$(q - 1) x_n = x_{n-1}$$

These equations give

$$-\frac{2\sin n\,\theta \sin\frac{1}{2}\theta}{\cos\frac{1}{2}\theta}\, x_n = \lambda\, x_0 = \lambda\, a \cos\mu\, t$$

$$x_r = \frac{\cos(n - r + \frac{1}{2})\,\theta}{\cos\frac{1}{2}\theta}\, x_n$$

$$= -\frac{\lambda\, a\, \cos\,(n - r + \frac{1}{2})\,\theta \cos\mu\, t}{2\sin n\,\theta \sin\frac{1}{2}\theta} \quad \ldots\ldots\ldots\ldots\ldots(8)$$

and the relative displacement is

$$x_{r+1} - x_r = -\frac{\lambda\, a \sin(n - r)\,\theta \cos\mu\, t}{\sin n\,\theta}$$

Let $\mu = 2\, m \sin\psi$, then θ, operating upon $\cos\mu t$, is equal to $2\,\psi$, and

$$x_{r+1} - x_r = -\frac{\lambda\, a \sin 2\,(n - r)\,\psi \cos\mu t}{\sin 2\, n\,\psi}$$

Now λ, representing the ratio of the action of an atom of ether to the mutual action of the atoms of a solid body, is very small, almost infinitesimal. The above coefficient of $\cos\mu t$ is therefore wholly insignificant except when $\sin 2n\psi$ is very small, or when $2\, n\,\psi =$ a multiple of π. In this case, as before explained, the quantity by which λ is multiplied is large, but not infinite: for as the amplitude of the vibration increases, the time of vibration is slightly increased, as indicated by the factor c, introduced in the second approximation; and ψ is thus slightly diminished. The only values of ψ which produce any sensible effect are $\frac{\pi}{2\,n}$, $\frac{2\,\pi}{2\,n}$, $\frac{3\,\pi}{2\,n}$, &c.......... Let $\psi = \frac{\upsilon\,\pi}{2\,n} = \upsilon\,\gamma$, where υ is an integer; putting $b = \frac{\lambda\, a}{2\sin n\,\theta}$, we have, supposing the n atoms to have been initially at rest, in their positions of stable equilibrium,

$$x_r = a_1 + \Sigma\, {}_r a_s \cos\mu_s\, t - \frac{b\cos(2\, n - 2\, r + 1)\,\gamma}{\sin\upsilon\,\gamma} \cos\mu t$$

where a_1 ${}_r a_s$ are arbitrary constants, and $\mu_s = 2\, m \sin s\,\gamma$. Putting $t = 0$

$$0 = a_1 + \Sigma\, {}_r a_s - \frac{b\cos(2\, n - 2\, r + 1)\,\upsilon\,\gamma}{\sin\upsilon\,\gamma}$$

Equation (8) gives

$${}_r a_s = {}_n a_s \frac{\cos(2\, n - 2\, r + 1)\, s\,\gamma}{\cos s\,\gamma}$$

whence it may be shewn that ${}_{\mathrm{n}}a_{\mathrm{s}}$, and consequently ${}_{\mathrm{r}}a_{\mathrm{s}}$, vanishes for all values of s except $s = \upsilon$, and

$$\frac{{}_{\mathrm{n}}a_{\mathrm{v}}}{\cos \upsilon\, \gamma} = \frac{b}{\sin \upsilon\, \gamma}$$

$$x_{\mathrm{r}} = \frac{b \cos (2\, n - 2\, r + 1)\, \upsilon\, \gamma}{\sin \upsilon\, \gamma} (1 - \cos \mu t)$$

$$= \frac{b \cos (2 n - 2\, r + 1)\, \psi}{\sin \psi} (1 - \cos\ \mu t)\ .$$

If the disturbance be supposed to cease, it may be shewn, as before, that the subsequent motion is represented by

$$x_{\mathrm{r}} = \frac{b \cos (2\, n - 2\, r + 1)\ \psi}{\sin \psi} \cos \mu t$$

so that the system radiates the same kind of heat which it absorbs. If μ be greater than $2m$, x_{r} is small, even in comparison with λa, when n is large; so that the system is incapable of absorbing or transmitting any heat for which μ is greater than $2m$.

Equation (8) gives

$$x_1 = \frac{b \cos (2\, n - 1)\ \psi}{\sin \psi} \ (1 - \cos \mu t)$$

So long as this coefficient is greater than a, the first of the n atoms continues to be accelerated by the ether. When steady motion is established, and the acceleration ceases, we have

$$\frac{b \cos\ (2\, n - 1)\ \psi}{\sin \psi} = a$$

$$x_{\mathrm{r}} = \frac{a \cos (2\, n - 2\, r + 1)\, \psi}{\cos (2\, n - 1)\ \psi} (1 - \cos \mu t)$$

$$= \frac{a \cos (2\, r - 1)\ \psi}{\cos \psi} (1 - \cos \mu t)$$

The principle which I have stated, if established, would afford some hope of our being able to understand the facts of "Spectrum Analysis." The fixed character of the bright lines makes it impossible to conceive that the energy due to the translation or rotation of the vapour molecules can have anything to do with their production. They must be caused by the internal vibrations of something of which the molecule is composed. A complete knowledge of the arrangement and mode of vibration of the atoms of a molecule, would involve a complete knowledge of the corresponding bright lines. We may imagine that the molecule of the very simple structure which

we have been considering, would give one bright line corresponding to its fundamental note; and fainter lines would correspond to some of the terms of the second and higher approximations.

It can hardly, I think, have escaped notice, that if the mean translation velocity of the molecules of an incandescent vapour become so great as to bear a sensible ratio to that of wave propagation, the wave length of the light corresponding to any bright line would be affected in a manner and degree depending upon the direction of motion relative to that of the light observed. As the temperatnre is gradually increased, this would have the effect of thickening the bright lines, and finally of converting them into a continuous spectrum. If as the temperature is increased, a rupture or change of constitution should take place in the molecules, we might expect a sudden change in the appearance of the spectrum. In reference to this subject, I may remark, although I express an opinion with much hesitation, knowing how much there is which has been written upon this subject, which I have not had an opportunity of studying, that I believe that the constitution of the luminiferous ether is such as to render it incapable of propagating waves of less than a certain length.

I see some hope also of an explanation of what has always appeared to me one of the greatest difficulties in connection with molecular physics; that the wave length should be so nearly the same for all kinds of heat. It is not difficult to conceive that the molecules in the sun and elsewhere, whose vibrations are the chief sources of heat, should have been so constituted as to vibrate nearly in the same time. The difficulty is to understand why the molecules of bodies of all kinds and constitutions, being heated and then left to vibrate in their own way, should all vibrate so nearly in unison. But if we hold that the arrangement of atoms into molecules is caused by the prevalent heat and depends upon its wave length, the difficulty disappears. Let us suppose for a moment that the sun should radiate heat of one uniform wave length only, and that the values of m for all substances and combinations were such that $\frac{\pi}{2\psi}$ were in every case a whole number exactly. All the atoms under the influence of the sun's heat would be arranged into molecules, all having the same fundamental note, and collections of such molecules after being heated, would give back that note alone. No substance having its atoms otherwise arranged could continue to exist, for every ray of heat which it encountered would assist in decomposing and rearranging its atoms according to the prevailing code. There would be one uniform stability of molecular constitution, and one uniform colour. μ appears to be as nearly constant as

the necessity that p should be a whole number allows. If μ were not nearly constant for heat of considerable intensity, there would be no stabilty in the constitution of matter; for an arrangement made under one wave length would be liable to be decomposed under another. Suppose, that a mass of any substance, such as iron, were brought from some other system, if there is any such, where a much longer wave length prevails. We should probably not recognize it as iron at all. If melted, its atoms would be immediately arranged according to the fashion of our system. It might perhaps be preserved in its original state if carefully kept in a cool place. If exposed to the heat of the day, it would probably be gradually transformed, suffering disintegration in the process—it would decay, in fact, much as a piece of wood does, and with more or less rapidity according to the degree in which its constitution differed from our standard. Is it possible that organic compounds, which can be produced and exist under exceptional circumstances only, which are so liable to decay, so sensitive to the action of heat, and differ sometimes so entirely from inorganic substances formed of the same chemical elements, may involve abnormal molecular arrangements, not in accordance with the prevailing wave length, and thus liable to decay when removed from the local influences under which they were produced?

The present uniformity of wave length, is a condition of dynamical equilibrium, which may have existed from the beginning, but which may, I think, have been brought about by the operation of natural causes. Supposing a number of atoms, enough to make a solar system, to have been created any where in space, but at such distances apart as to cause by their confluence, a sufficient amount of heat to animate the whole. In the beginning there would be a true chaos. There would be every variety of wave length, and consequently every variety of molecular arrangement, with no stability any where, but a continuous process of composition and decomposition. But out of this chaos order would be gradually evolved. The principle of natural selection would begin to operate even at this early period. Every radiating molecule would endeavour to impress its own constitution upon others within its influence, to propagate its kind. In the warfare among the molecules, every enemy conquered would become the ally of the conqueror. The molecules distinguished by numbers and strength of constitution, would gradually gain the ascendancy by the destruction of weaker kinds; and any additional stability of structure which might accidentally arise amongst themselves would be propagated and become general. An ascendancy having once been gained, the process of reduction to a common standard, would go on with an ever increasing ra-

pidity, until the condition of greatest stability was attained. If such a relation existed amongst the constants upon which the mutual action of the atoms depends, as to render it possible that one uniform wave length should be attained, that would be the final result. In that case there would be one uniform stability of molecular arrangement; a hard uncompromising state of things, without the possibility perhaps, of that continuous round of composition and decomposition upon which the life of our part of the Universe depends.

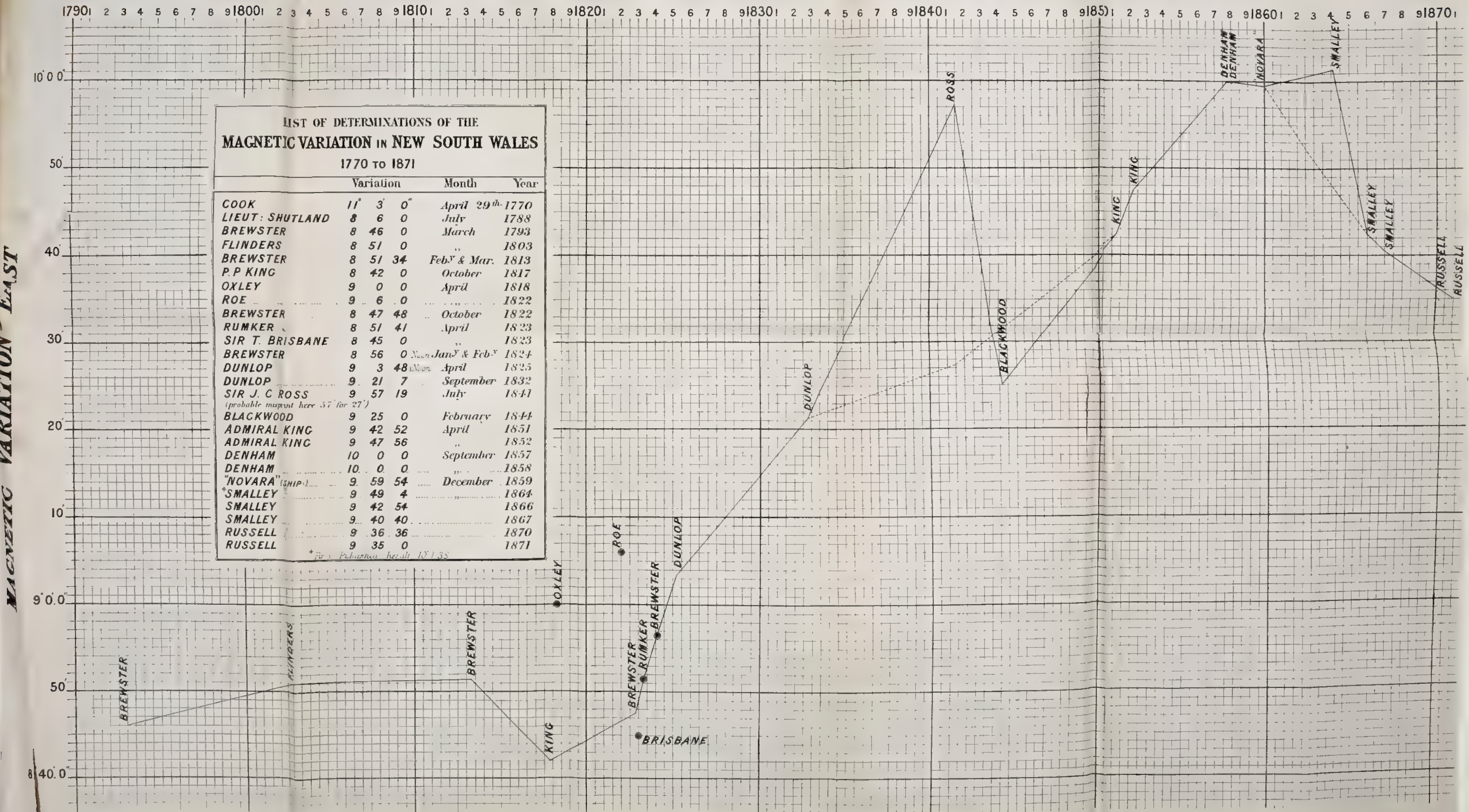
It may be, then, that chaos means diversity of wave length, and that cosmos means variety in unity, and that absolute uniformity of wave length would be universal death. It is a curious subject for reflection, that the possibility of cosmos evolving out of chaos; that is, the possibility that the material universe should become fitted to be the abode of organic life, may have depended upon whether or not a few constants were so arranged, in the beginning, as to satisfy a simple mathematical condition.

Joseph Cook and Co., Machine Printers, 370, George-street, Sydney.

NETIC VARIATION
EAST

No 1

DIAGRAM OF MAGNETIC VARIATION AT SYDNEY 1793 TO 1871



LIST OF DETERMINATIONS OF THE MAGNETIC VARIATION IN NEW SOUTH WALES 1770 TO 1871

	Variation			Month	Year
COOK	11°	3′	0″	April 29th	1770
LIEUT: SHUTLAND	8	6	0	July	1788
BREWSTER	8	46	0	March	1793
FLINDERS	8	51	0	,,	1803
BREWSTER	8	51	34	Feby & Mar.	1813
P. P KING	8	42	0	October	1817
OXLEY	9	0	0	April	1818
ROE	9	6	0	,,	1822
BREWSTER	8	47	48	October	1822
RUMKER	8	51	41	April	1823
SIR T. BRISBANE	8	45	0	,,	1823
BREWSTER	8	56	0	Noon Jany & Feby	1824
DUNLOP	9	3	48	Noon April	1825
DUNLOP	9	21	7	September	1832
SIR J. C ROSS (probable misprint here 57′ for 27′)	9	57	19	July	1841
BLACKWOOD	9	25	0	February	1844
ADMIRAL KING	9	42	52	April	1851
ADMIRAL KING	9	47	56	,,	1852
DENHAM	10	0	0	September	1857
DENHAM	10	0	0	,,	1858
"NOVARA" (SHIP)	9	59	54	December	1859
*SMALLEY	9	49	4		1864
SMALLEY	9	42	54		1866
SMALLEY	9	40	40		1867
RUSSELL	9	36	36		1870
RUSSELL	9	35	0		1871

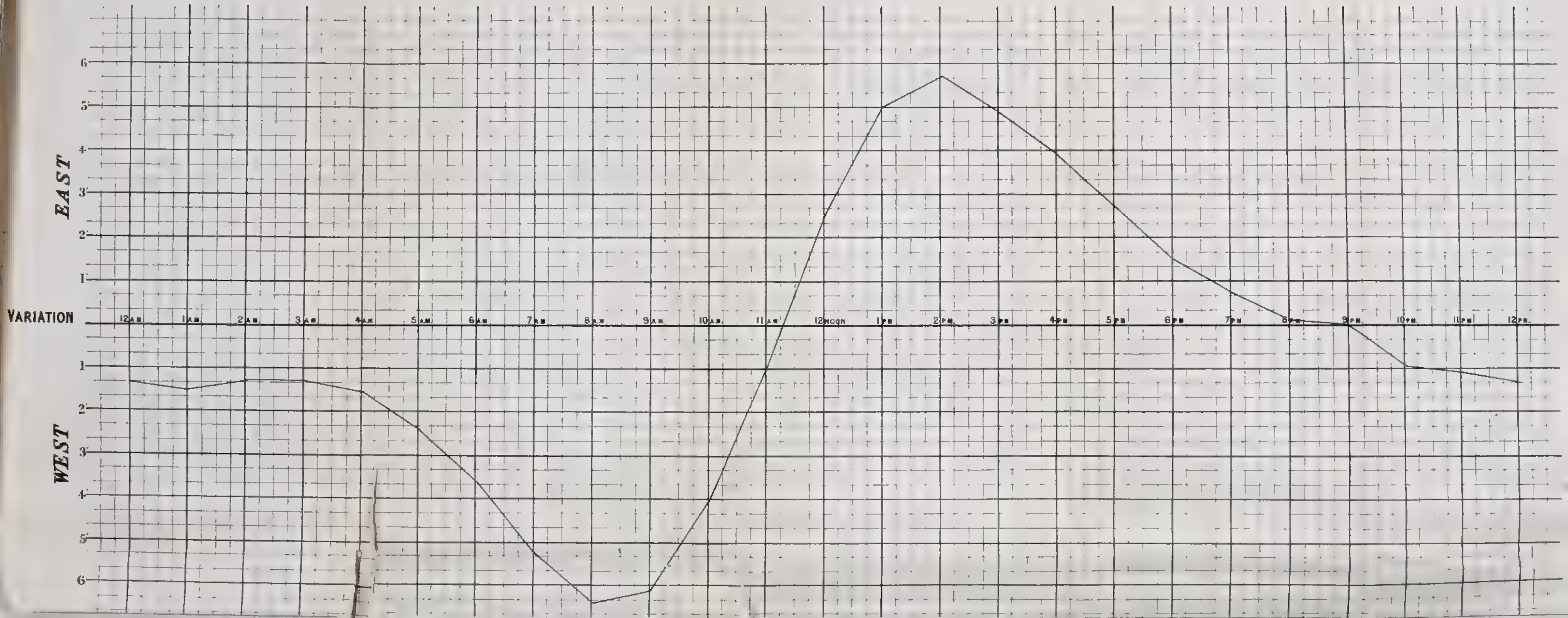
* [illegible]

AVERAGE DIRECTION OF THE MAGNETIC NEEDLE

AT EACH HOUR DURING THE MONTH OF

JANUARY

Nº 2

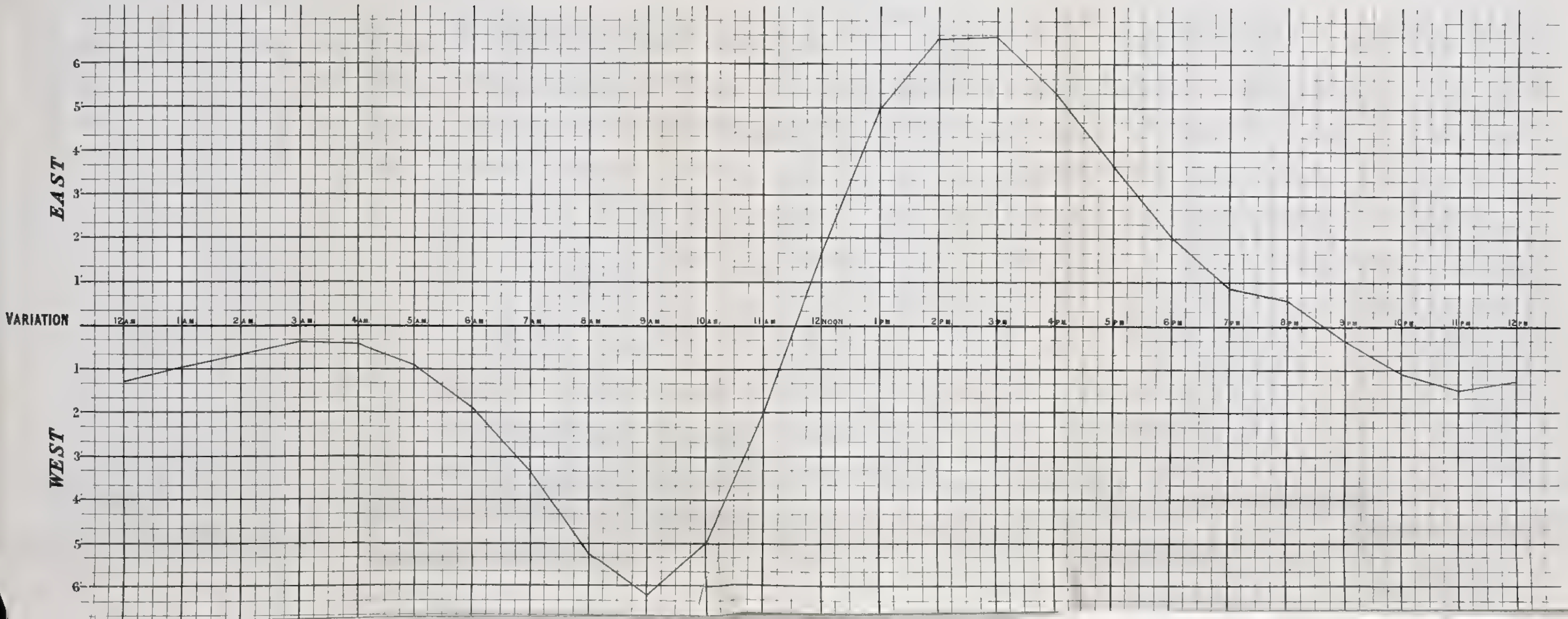


AVERAGE DIRECTION OF THE MAGNETIC NEEDLE

AT EACH HOUR DURING THE MONTH OF

FEBRUARY

Nº 3

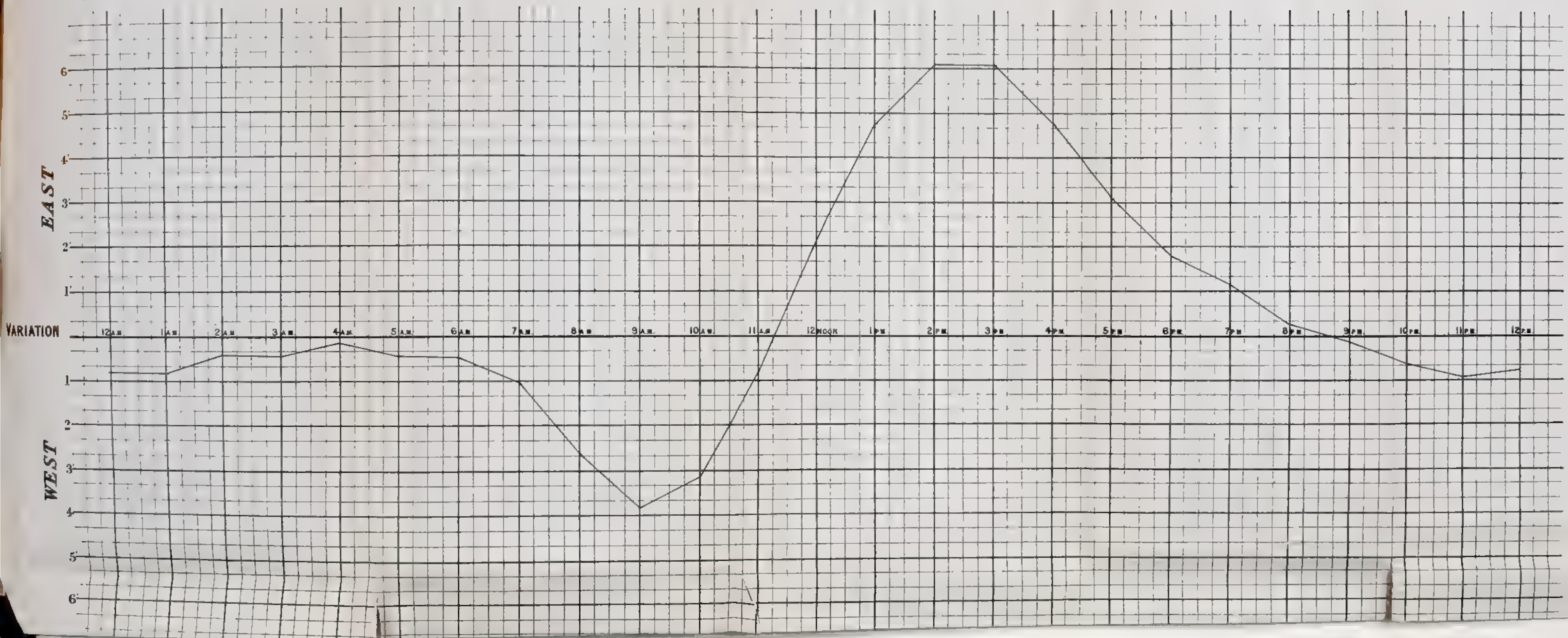


AVERAGE DIRECTION OF THE MAGNETIC NEEDLE

AT EACH HOUR DURING THE MONTH OF

MARCH

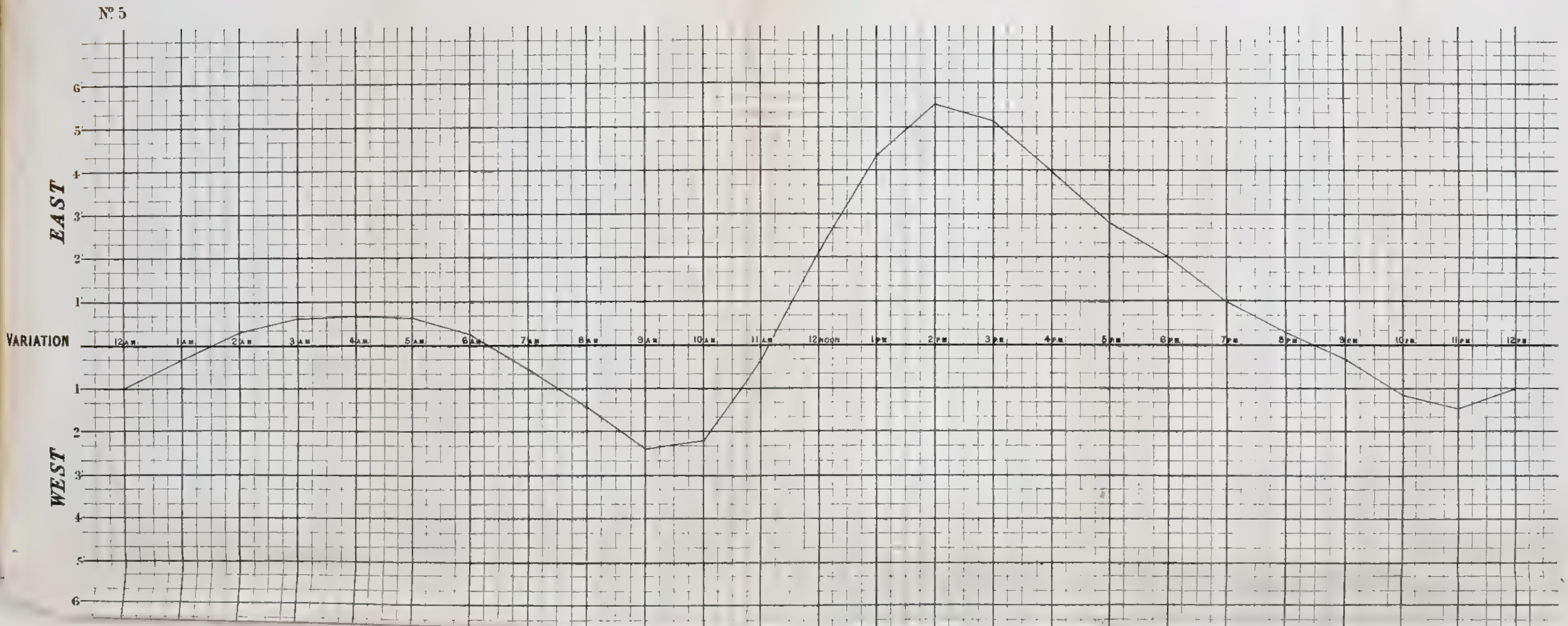
Nº 4



AVERAGE DIRECTION OF THE MAGNETIC NEEDLE

AT EACH HOUR DURING THE MONTH OF

APRIL

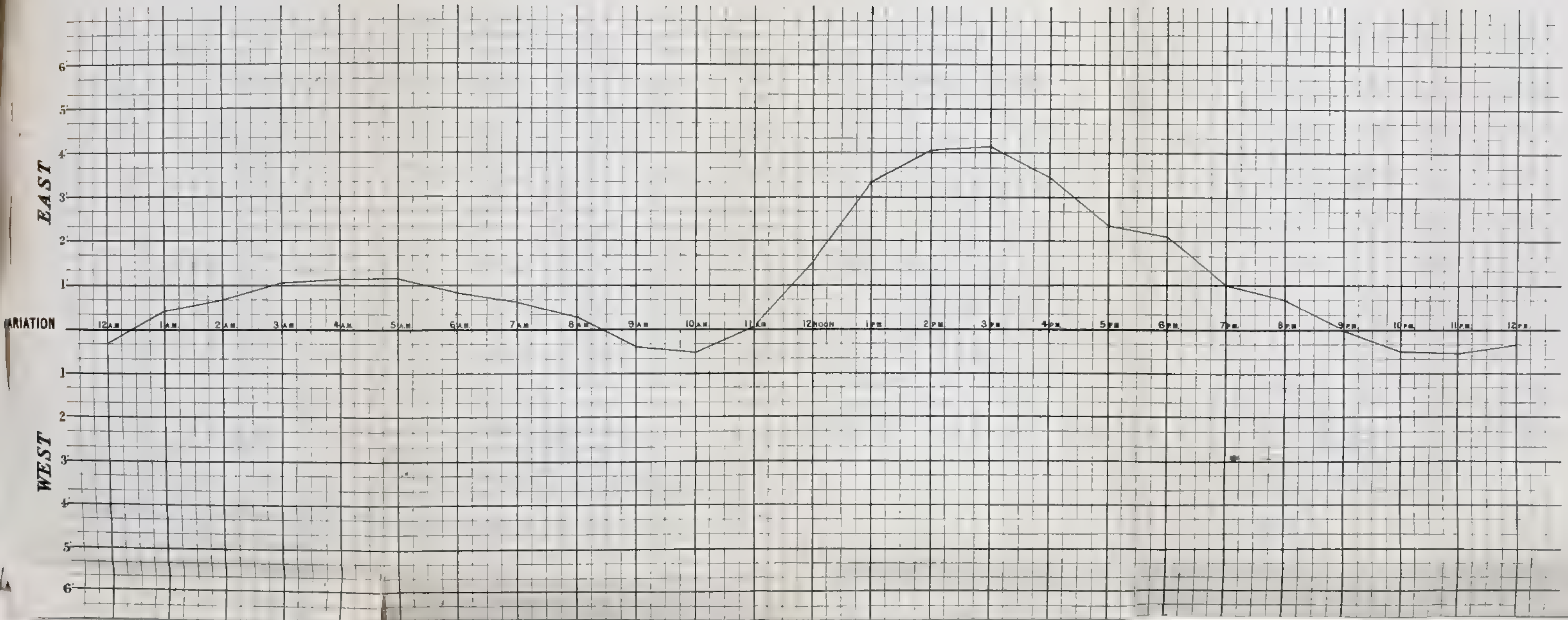


AVERAGE DIRECTION OF THE MAGNETIC NEEDLE

AT EACH HOUR DURING THE MONTH OF

MAY

Nº 6

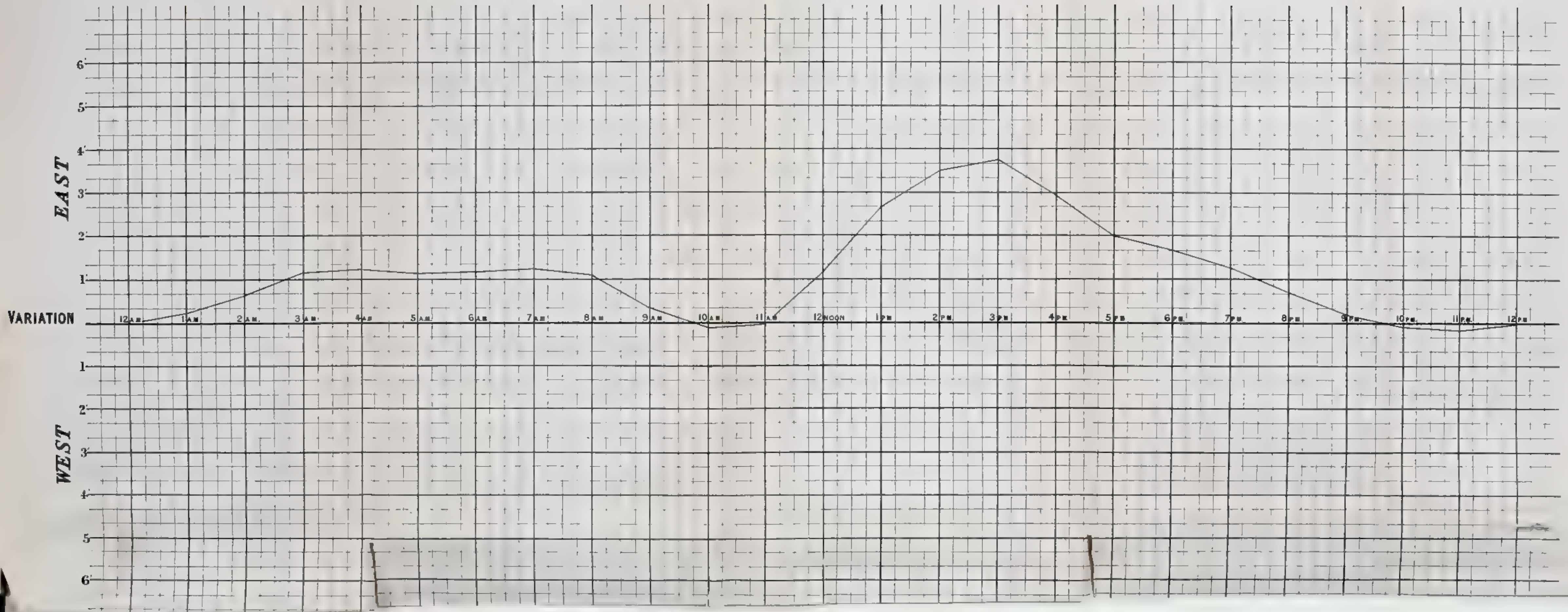


AVERAGE DIRECTION OF THE MAGNETIC NEEDLE

AT EACH HOUR DURING THE MONTH OF

JUNE

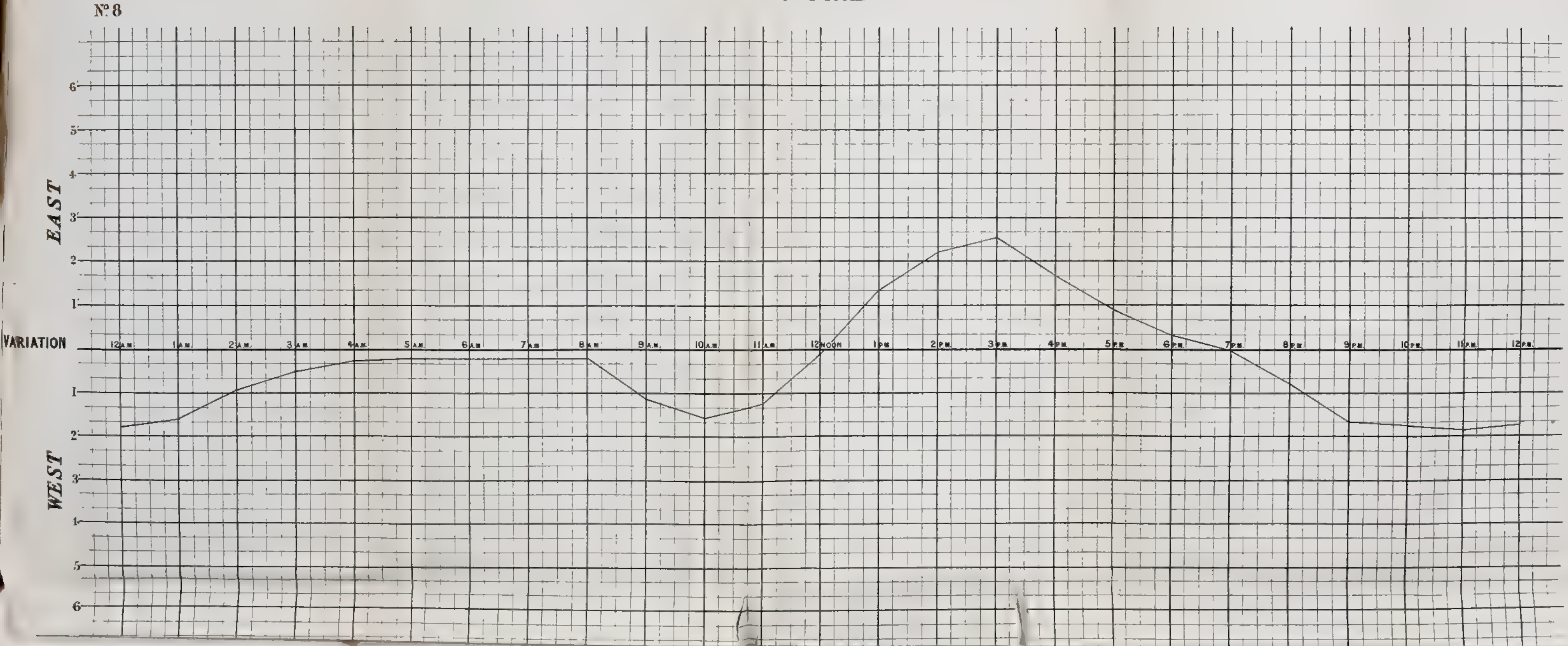
Nº 7



AVERAGE DIRECTION OF THE MAGNETIC NEEDLE

AT EACH HOUR DURING THE MONTH OF

JULY

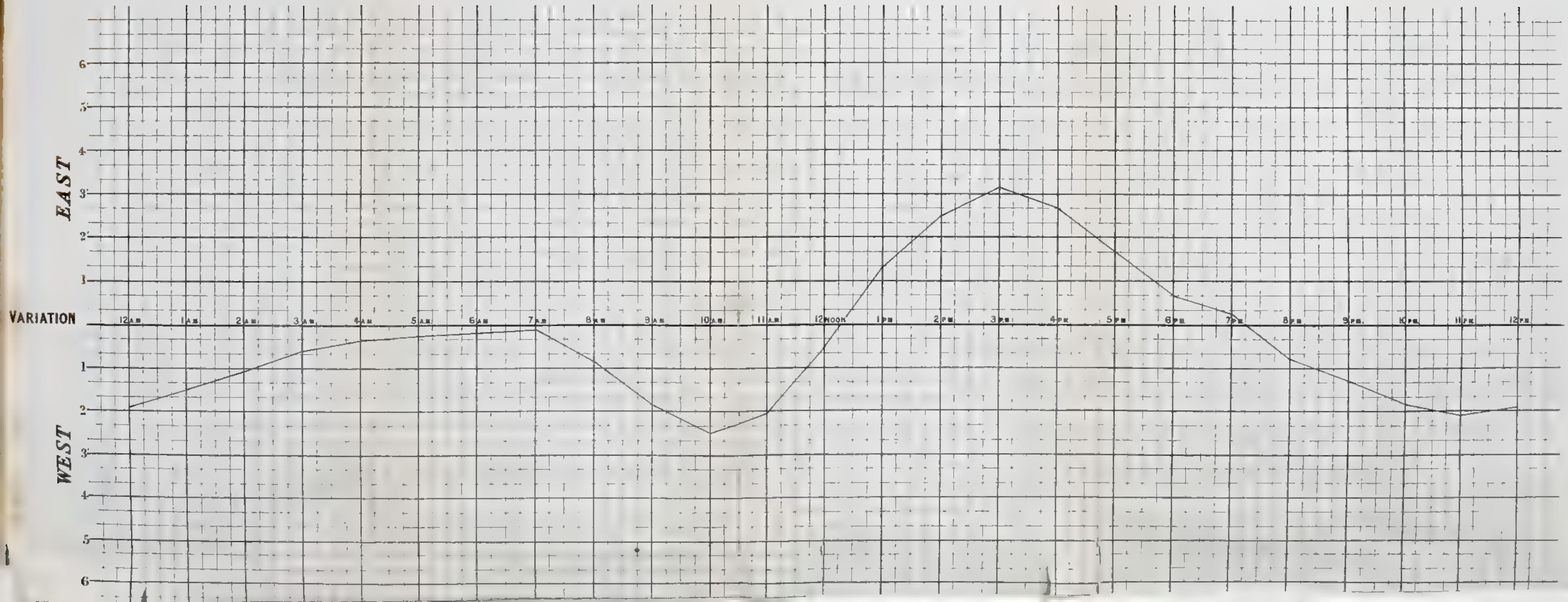


AVERAGE DIRECTION OF THE MAGNETIC NEEDLE

AT EACH HOUR DURING THE MONTH OF

AUGUST

Nº 9

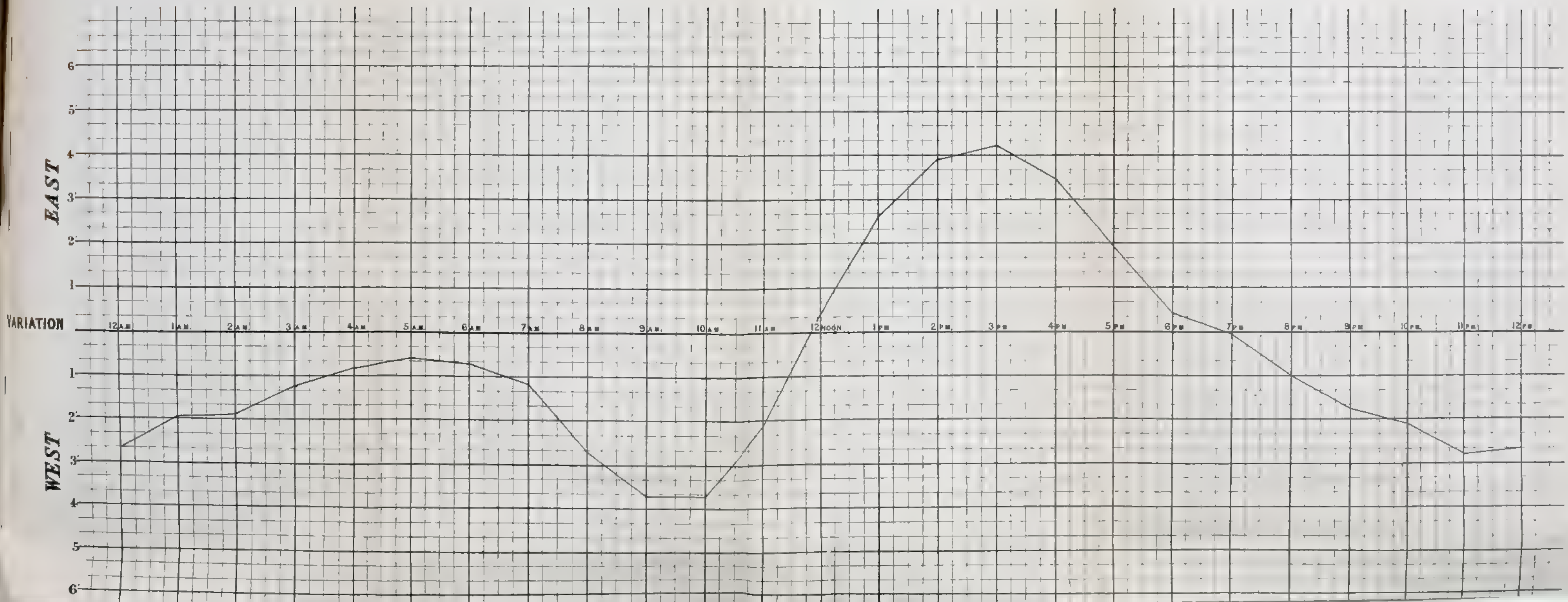


AVERAGE DIRECTION OF THE MAGNETIC NEEDLE

AT EACH HOUR DURING THE MONTH OF

SEPTEMBER

Nº 10

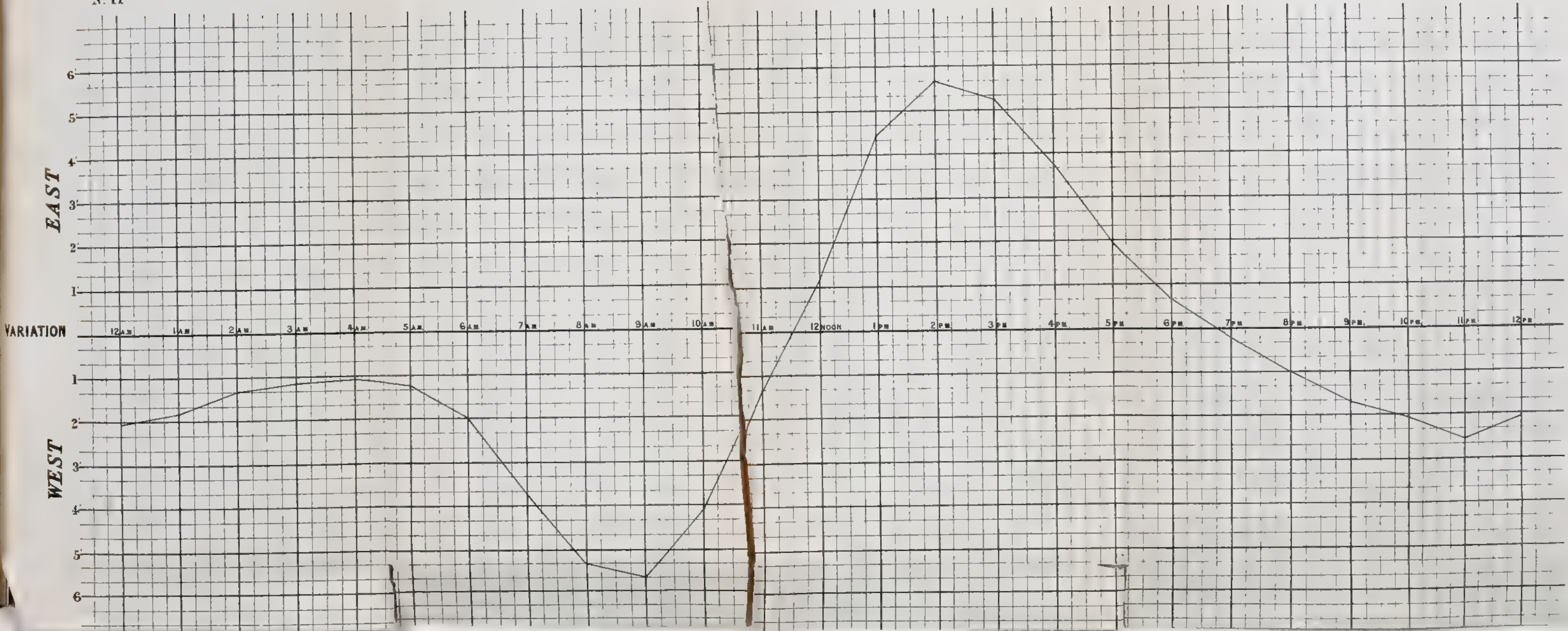


AVERAGE DIRECTION OF THE MAGNETIC NEEDLE

AT EACH HOUR DURING THE MONTH OF

OCTOBER

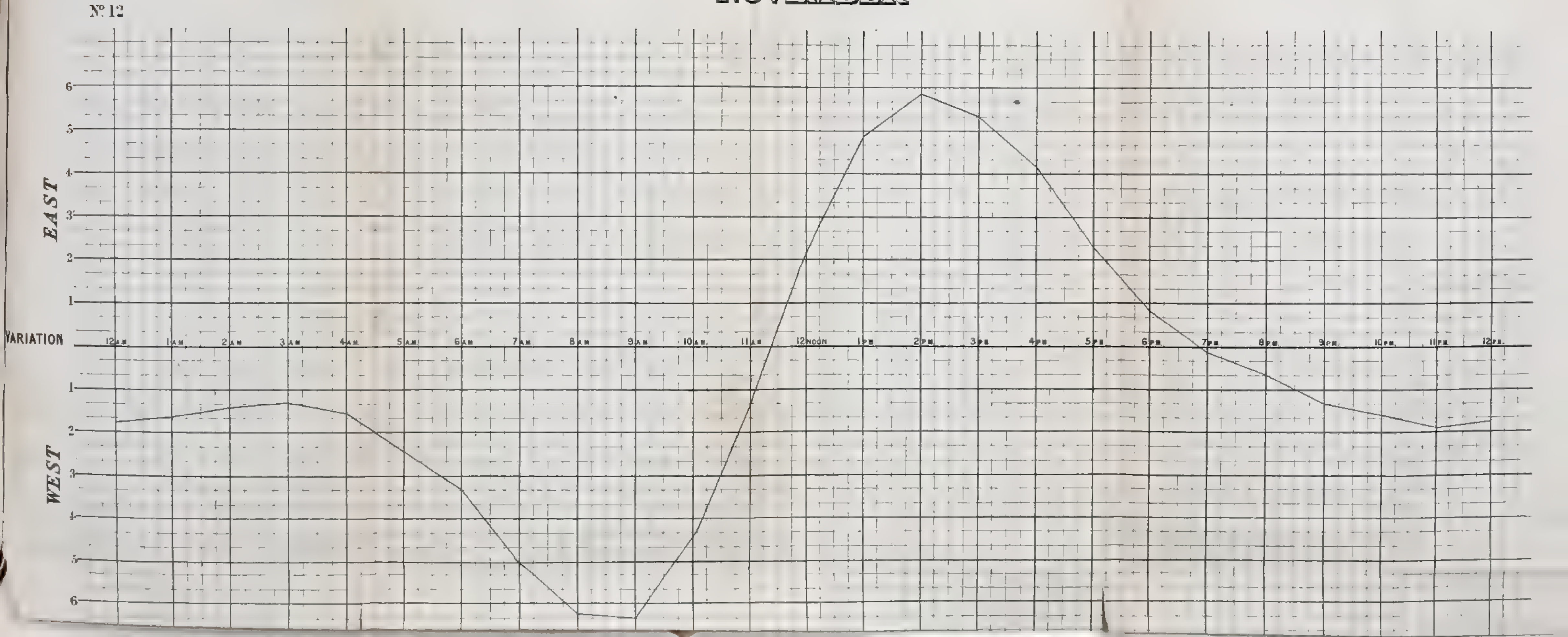
Nº 11



AVERAGE DIRECTION OF THE MAGNETIC NEEDLE

AT EACH HOUR DURING THE MONTH OF

NOVEMBER



AVERAGE DIRECTION OF THE MAGNETIC NEEDLE

AT EACH HOUR DURING THE MONTH OF

DECEMBER

No. 13